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## ORIGINAL ARTICLES.

### ANOTHER VIEW OF CONSERVATIVE SURGERY OF THE TUBES AND OVARIES.<sup>1</sup>

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THE history of gynecological surgery in America during the past fifteen years has been marked by a series of radical changes which, to a foreign observer, must doubtless seem not a little puzzling. Three or four years have often sufficed to effect an entire transformation of the opinions and practice of our representative surgeons. The reproach that we have been carried to and fro with every wind of doctrine has not always been undeserved. You will recall how promptly we adopted the views of Lawson Tait, the operation to which his name is attached being performed hundreds of times within a few months after it was introduced into this country. Every foreign surgeon of note who has visited our shores has left his impress upon us, and has raised up a host of imitators, no matter how radical may have been his methods. From one extreme to another the pendulum has swiftly vibrated, rarely stopping in the middle of its swing. With all these changes in surgical methods this Society has been intimately associated. The hard-fought battles in which we have engaged, honest and manly, and without any tinge of bitterness, have not been in vain. A most commendable trait in our Fellows has been their readiness to correct their errors by the light of experience. A review of our *Transactions* will convince the impartial reader that, although we have kept fully abreast of modern methods, even the most radical, some of our best work has been in the line of conservative surgery. I need only to allude to the valuable papers in this line by eminent authors, some of whom will meet with us no more.

I approach this subject in a spirit far from pessimistic, feeling that, as yet, we are probably only groping after the truth. Ten or twenty years from now others will see clearly what we now see "through a glass darkly." We are all aiming at one end, and if at times we seem to hold contrary views, so much the better; the truth will sooner appear.

No man has a greater sympathy with the conservative movement in pelvic surgery than I; in fact,

my position was once exceedingly unpopular. When one recalls how a few years ago at the operating-table an ovary was drawn out of the abdominal cavity, and when found to be a little larger than normal, with a few cysts on its surface, it was called "cystic," or, if it had undergone normal atrophy, was pronounced to be "cirrhotic"—and in either case was promptly removed—one feels that there is great cause for thankfulness that we have learned better. If such an ovary was adherent, so much the more reason was there for removing it, without much regard to its macroscopic appearance. The familiar diagnosis "catarrhal salpingitis" was not infrequently a sufficient excuse for extirpating a tube which was possibly the seat of ordinary menstrual congestion. Most of us must plead guilty to such errors in the past, and we rejoice that this kind of surgery has been banished, never to return.

It is my purpose to inquire briefly whether we are not in some danger of going to the other extreme. The word "conservatism" is often on our lips; in fact, we have come to use it somewhat loosely. Baldy expresses the views of not a few when he says: "I have no objection to conservatism when I know what it means." It is not necessary to review the literature of this subject, with which you are all familiar, notably the papers of our esteemed Fellows. Now that we have had further experience with the matter it is not improper to ask it the results of conservative surgery of the adnexa are all that they have been represented to be. In the early, or what may be called the statistical, stage of abdominal surgery the test of the successful operator was simply recovery from the operation. Now, it is the relief of symptoms, or the restoration of impaired functions. In other words, we look to ultimate rather than to immediate results. This is an essential difference which, in itself, marks the progress of our art; theory has given place to practice. Once the death of the patient was a proof of failure; now, the persistence of pain and other distressing symptoms, for the relief of which the operation was performed, are evidence of some error in diagnosis or technic.

I do not intend to waste the time of this intelligent audience in defining what we understand by conservative surgery of the tubes and ovaries. As I understand it, it consists essentially in the preservation of a sufficient amount of ovarian tissue to con-

<sup>1</sup> Read at the Twenty-third Annual Meeting of the American Gynecological Society, held at Boston, May 24, 25, and 26, 1898.

tinue the functions of menstruation and ovulation, and, at the same time, by maintaining the patency of the Fallopian tube, to render impregnation possible. The question of priority in this field, as in every other, is a matter of secondary importance. Martin admits that Spencer Wells punctured dropsical follicles as far back as 1864, while I recall that this was a common practice with Dr. T. G. Thomas many years ago. The work of Martin, Schroeder, and Pozzi abroad, and of Polk, A. P. Dudley, and many others in this country, is well known.

What, may we ask, is the true motive for conservative work in this direction? The answer may be given from the standpoint of the patient, or from that of the surgeon. A young woman naturally shrinks from the thought of losing both her ovaries. If she is unmarried, the fear that castration may lead to essential changes, physical and mental, is one which we can no longer regard as purely sentimental. The belief that profound disturbances commonly follow this operation is too deeply rooted in the minds of the laity to admit of argument. I need only refer to the last classical paper of our lamented Goodell, whose dictum in regard to the advisability of preserving the function of menstruation whenever possible has been accepted by all thoughtful men. If a woman is married, but has never borne children, the hope of future pregnancy is often so strong that she is willing to endure any amount of suffering rather than to be relieved at the cost of a sacrifice of the prospects of maternity.

For the surgeon the problem is one based upon his previous experience with the ultimate results of entire removal of the adnexa. The disturbances attending the artificial climacteric are so familiar to us that there is no one present who does not recall cases in which he honestly believes that the patient would have been better off if he had not operated at all.

Granting that the severity of these phenomena, especially the relative frequency of insanity following castration, have undoubtedly been exaggerated, the fact still remains, as Goodell has so emphatically stated, that castration is often followed by deplorable consequences. We are glad to think that these bitter words which he wrote five years ago are no longer true: "The removal of the ovaries has developed into a busy industry, by which in city and in country, very many women have been, and are being, mutilated, both needlessly and on the slightest provocation." By preserving the function of menstruation we certainly avoid many of these unpleasant sequelæ. As regards the question of future pregnancy, the surgeon is naturally more indifferent than the patient. From his stand-

point, the removal of diseased organs and the relief of distressing symptoms is the great desideratum. If pregnancy may occur later with perfect safety to the patient, so much the better. He cannot, however, avoid being strongly biased by her earnest wishes in the matter.

You are familiar with the technic of the operation in question. We all know how to puncture or to excise small cysts of the ovary, to remove portions of the gland which present macroscopic evidences of disease, to break up adhesions, to catheterize and to resect tubes, to perform "salpingopexie," as Pozzi calls it—that is, to attach the ovary or a portion of it to the stump of the resected tube. Some of us have even gone so far as to preserve portions of pus-tubes, of purulent ovaries, of dermoid cysts, or of larger cysto-adenomata, which seemed to contain normal stroma. The vital question is, in performing these operations, Are we certain that we always accomplish the results aimed at? Do we relieve pain, ward off immediate or future danger from the patient, preserve the functions of menstruation and ovulation, and cure sterility, or, at least, favor conception in a woman who would not otherwise have become pregnant except for our interference? These are difficult questions to answer; at least our data are not sufficiently numerous to justify us in answering them dogmatically. It is a mark of our fallible nature that we are prone to see what we wish to see. The enthusiastic advocate of a certain method of overcoming stenosis of the cervical canal reports a long list of cases in which conception followed his peculiar method of operation, or the insertion of a specially devised stem pessary. But how many of these patients would have become pregnant if no operation had been performed?

I need not remind you, with your wide experience, of the singular manner in which the ovaries continue to discharge their functions and conception takes place under conditions which, to the human mind, seem impossible. Who has not seen menstruation, in every respect regular, occurring in women with cancer of both ovaries, with double ovarian abscesses, with large cystomata, when the most careful search fails to disclose any macroscopic evidences of normal stroma? Who has not observed pregnancy in women with advanced carcinoma of the cervix uteri? We cannot explain these things. They only show how often Nature works successfully under the most unfavorable conditions. The idea is a fascinating one that we can deal with abnormal conditions in the pelvis just as a skilful jeweler repairs a damaged watch; that, by removing a few adhesions here, excising a little tissue there, restoring the patency of an occluded canal, we can read-

just the delicate mechanism, and, as it were, restore lost functions.

The simplest form of conservative treatment consists in separating the adhesions around tubes and ovaries which present few if any macroscopic evidences of disease. This is, undoubtedly, a most valuable procedure, since these adhesions are often the cause of abdominal pains entirely out of proportion to the local lesion. My experience is certainly not different from that of my hearers when I say that, while many of these patients have been greatly benefitted as regards the relief of both dysmenorrhea and of persistent pains, the adhesions have often reformed. It is safe to say that up to the present time no method of absolutely preventing the reformation of such adhesions has yet been discovered. They occur after the simplest aseptic celiotomies; in fact, it is the rule to meet with them in secondary operations. Although I have practised the method frequently, I have always regarded the mere separation of such adhesions by vaginal section as an operation of no permanent value, especially when the ovaries were prolapsed in Douglas' pouch. They naturally tend to resume their former position, and to become fixed by fresh adhesions. It has seemed to me much more rational to employ the abdominal route, and, after separating an adherent ovary, to suture it near the pelvic brim. Conception has undoubtedly followed this simple procedure, but one can instance quite as many cases in which the patient became pregnant with one or both ovaries buried in adhesions.

A further step in the direction of conservatism is the puncture or excision of small cysts on the surface of the ovary. This seems to carry us back to the early days, when we used to remove such ovaries simply on suspicion. Surely, we have learned a little pathology since then; among other things, the fact that such cystic ovaries are rarely the forerunners of large cysto-adenomata, and that dropsical follicles, the size of marbles, may still contain healthy ova. Pozzi has written much on the subject of igni-puncture of sclerotic ovaries. But, in spite of the amount of honest work which has been expended on the histology of the ovary, we are not yet in a position to define strictly the limits between normal and pathologic cirrhosis in the stroma. Doubtless in the hasty inspection at the operating-table corpora fibrosa are frequently mistaken for inflammatory thickening. This being the case, how do we know, when we puncture or excise a cyst, or an area of supposed cicatricial tissue in the ovary, that we are really doing the patient a service by removing a pathologic condition? That she becomes pregnant afterward is hardly a proof that the operation was a direct cause of

pregnancy. Invest the procedure with high-sounding terms as we may, it still remains an experimental attempt to remove what we only suspect may be a cause of future trouble. Curettement is usually performed as a preliminary step, so that in successful cases there must always be considerable doubt as to whether this or the abdominal operation was the true factor in overcoming sterility.

It has been assumed by those who have written on this subject that the risks in ovarian and tubal resection are practically *nil*, but the writer is personally cognizant of a case in the practice of an eminent surgeon in which secondary hemorrhage followed the excision of a follicular cyst per vaginam. It was necessary to open the patient's abdomen several hours after the operation in order to check the bleeding, and her escape from death was little short of miraculous. In the case of small purulent and dermoid cysts of the ovary, excision of the diseased portions and suture of the raw surfaces is certainly not entirely free from danger, since one cannot be absolutely certain that septic foci are not left behind. Délanger, one of the enthusiastic followers of Pozzi, admits that it is practically impossible to remove all the diseased tissue, and that we can only hope to reduce it to a minimum. Pozzi's theory that the action of the cautery causes the absorption of sclerotic tissue is largely *theoretical*; in fact, he limits the application of this method to what he calls "microcystic degeneration, and diffuse or edematous oophoritis." Landau calls attention to the well-known anatomic fact that atresia following gonorrheal salpingitis is really a conservative process of Nature, and asks, quite pertinently, why, then, we should open up such tubes and expose the patient to the risk of a fresh attack of perisalpingitis, or even of death from infection? It is an elementary fact that unless a bacteriologic examination is made during the progress of the operation we are not in a position to affirm with any degree of positiveness concerning the infectious character of the fluid contained within an occluded tube. Pozzi not only asserts that fatal cases of sepsis have been reported after resection of the tubes, but believes that the reason why more women do not die is presumably due to the fact that the distal ends of the tubes again become agglutinated, or else are surrounded by fresh adhesions.

In my experience, which now includes more than thirty cases (nearly all within the past three years), the immediate ill-effects of the operation have not been such as to prejudice me against it on the ground of the added risk. The question as to the subsequent condition of the patient is more important. In all the cases in which I have operated it has been for the purpose of



relieving severe pain and dysmenorrhea, and removing adnexa which were hopelessly diseased, or were the seat of small neoplasms. Most of these patients have been kept under observation, and with some of them it has been necessary to perform a secondary operation. This experience has been supplemented by secondary celiotomies performed upon patients who had previously undergone conservative operations at the hands of other surgeons. Doubtless this experience is not an uncommon one. As regards relief of symptoms, the following points have been noticed:

*Relief of Pain.*—In several instances in which dense adhesions were separated, the tube and ovary on one side were removed, and on the opposite side were resected and secured as nearly as possible in their normal position, the results were not as satisfactory as could be wished. The adhesions evidently reformed, the patient suffered from practically the same pain as before, and but slight difference was noted as regards dysmenorrhea. In all these cases the convalescence was afebrile, no drainage was employed, and, with few exceptions, absorbable sutures and ligatures were used. An examination of several of these patients at intervals of from three to twelve months after operation showed the presence of a painful induration at the site of the resected ovary and tube. In only one instance, thus far, has pregnancy occurred and is now progressing normally, although the patient suffered from considerable local pain and menorrhagia for several months after the operation.

*Menstruation.*—The variations in menstruation noted when a portion of the ovary, including from one-fourth to two-thirds of the normal stroma, was left behind, have been many. In two instances menstruation, after returning slightly two or three times, ceased entirely. In other cases the flow was slight and irregular, but persistent; in two more profuse than before, and attended by pain several days before and during the flow. In my experience the relief of dysmenorrhea has been gradual rather than immediate.

This result is hardly surprising in view of the fact that the portion of the ovary which remains has been known to atrophy entirely, or to undergo cystic degeneration. Cases in which a secondary operation has been necessary for the removal of a cystoma, developing from the remains of the stroma, or of a tubo-ovarian cyst, have been reported by Martin and others. I showed such a specimen at a recent meeting of the Obstetrical Society. Chrobak's objection, that there is great danger of ectopic pregnancy occurring in a resected tube has, fortunately, proved to be a theoretical one so far as I am aware.

The following brief notes of cases will serve to illustrate some of the points which have been made:

CASE I.—This illustrates the danger of attempting to preserve an apparently healthy ovary when the opposite one is the seat of papilloma.

The patient was a young woman, aged twenty-five years, who was engaged to be married. Her physician's attention was called to her large abdomen and rapidly declining health. On examination I found a considerable ascitic accumulation; there was a small mass at the site of the right ovary. Explorative incision; several pints of ascitic fluid evacuated. The right ovary was moderately enlarged with a few papillomatous masses on its surface. No adhesions. A few papillomata the size of millet seeds were seen on the pelvic peritoneum. The opposite ovary was carefully examined, and to all appearances was perfectly normal. In view of the patient's circumstances it was judged wiser not to remove it. She recovered, married a few months later, and remained well for a year, when she again developed ascites. A second abdominal section was performed and the papillomatous left ovary was removed. She recovered, and is well at the expiration of a year.

It may be objected that this was an improper case for conservatism; and yet, judging from the condition of the opposite ovary and tube, it seemed justifiable, under the circumstances, to spare them.

CASE II.—Married woman, aged twenty-six years; separated from her husband. Suffered from constant pain and dysmenorrhea. History of recurrent attacks of pelvic peritonitis, presumably of gonorrheal origin. Uterus in normal position. Both ovaries and tubes considerably enlarged, very tender, and fixed by adhesions. The patient was desirous of marrying again, and anxious that one ovary should be saved, if possible.

Vaginal section. Adhesions easily broken down. Pyosalpinx and small ovarian cyst on the left side tied off with catgut. On the right side was a cystic ovary and a large and thickened tube, the distal end being occluded. One-half the ovary was resected, also the outer third of the tube; the tube was catheterized and the mucous membrane stitched to the peritoneum in the usual manner. Convalescence afebrile. The patient's condition after operation instead of being improved was so much aggravated that, finding local treatment, electricity, etc., of no avail, I proposed abdominal section, especially as a mass the size of an orange had developed at the site of the remaining ovary. The abdomen was opened a year after the first operation. No trace of the left ovary and tube and few adhesions. On the right side was an ovarian cyst, the size of a small orange, to the surface of which was adherent the resected tube, with its end occluded. The specimen was carefully examined, and it was found that the cyst had developed from the remains of the ovary, a small portion of the stroma being still visible. The tumor was thoroughly removed and the tube ex-



sected. Convalescence afebrile. Patient's condition after operation worse than before. She menstruated more or less regularly, using twenty-four napkins, though previously the flow had been rather scanty. A year has elapsed since the second operation. She writes me within a week that although her general health is better she has practically the same pain as before and flows more profusely than ever, so I regret exceedingly that I did not remove the uterus.

CASE III.—Conservative operation. Patient aged twenty-four years; married three years; never pregnant; anxious to have a child. Sharp ante flexion of the uterus, with stenosis. To the left of the uterus a cyst the size of a large orange. Divulsion and curettement. Broad ligament cyst on the left side removed with the ovary, which was atrophied. On the right side was a cystic ovary the size of an English walnut; small hydrosalpinx. Numerous adhesions were separated, half the ovary excised and sutured with catgut, the occluded tube opened, catheterized, and the mucous membrane stitched to the peritoneum. The ovary and tube were secured near the upper border of the right broad ligament.

Convalescence afebrile. Patient has now been under observation for six months. She has persistent pain, so that she can take but little exercise, although formerly she could ride a bicycle. Examination shows a tender mass at the site of the right ovary. Dysmenorrhea the same as before. Patient somewhat relieved by the use of ichthyol tampons and electricity, but regrets that she did not have both ovaries removed.

CASE IV.—Mrs. Q., aged thirty-five years; married eight years; never pregnant. A year before one ovary and a small fibroid were removed by the late Dr. Lusk. Patient suffered from persistent pain, dysmenorrhea, and menorrhagia. Examination showed the uterus small and in normal position. Douglas' pouch was occupied by an enlarged, tender ovary, firmly adherent.

Vaginal section. Cystic ovary was drawn down, and was supposed to have been entirely removed with the tube.

Afebrile convalescence. Patient continued to have persistent pain. Had profuse, irregular hemorrhages, which rendered her practically an invalid. She desired to have her uterus removed. It was done by abdominal section six months after the vaginal operation. Numerous pelvic adhesions were broken up. There was no trace of the left ovary or tube; the right tube had been entirely removed. At the site of the right ovary was a cyst the size of an English walnut, which had apparently developed from a small portion of the stroma which had been included in the ligature. Convalescence normal. Patient discharged, cured.

CASE V.—Illustrating the fact that menstruation does not always continue if a portion of the healthy ovary is left.

Married woman, aged thirty years; one child. Operation on account of persistent pain and dys-

menorrhea; cystic tumor the size of a grape-fruit to the left of the uterus. This was found to be a simple cyst of the broad ligament, with atrophy of the ovary. Tube normal. Cyst and ovary were removed. On the right side was found a large hydrosalpinx, which was removed, and an ovary, the seat of general cystic degeneration. In order to preserve the function of menstruation, about one-third of this ovary was left, including stroma which was apparently healthy and contained normal follicles. Afebrile convalescence. Six months have elapsed since the operation. The patient has not menstruated, and has the usual climacteric disturbances following castration.

CASE VI.—Persistent menstruation following supposed removal of both ovaries and tubes.

Married woman, aged twenty-seven years; one child. Operation on account of cystic degeneration of both ovaries, with adhesions, causing constant pain and dysmenorrhea. Unrelieved by a long course of local treatment, electricity, etc.

Operation uncomplicated; convalescence afebrile; silk ligatures used.

In this case a careful attempt was made to remove all the ovarian tissue on both sides. Menstruation has persisted, at irregular intervals, for five or six years, attended by more pain than before. An induration the size of a marble can be felt at the site of the stump on the left side. Patient has not been relieved by local treatment, but refuses a second operation.

In this case it is fair to assume that a small portion of ovarian stroma was included in the ligature, around which an inflammatory exudate formed.

CASE VII.—Repeated abdominal section in the same patient.

Unmarried woman, aged twenty-four years. Original trouble probably gonorrheal. Her right tube and ovary were removed by a colleague a year before. At this time the adnexa on the left side were supposed to be normal, and were simply freed from adhesions. The patient was unrelieved, and a year later came under my care. I found a mass on the left side which was extremely tender. She suffered from dysmenorrhea and profuse menstruation. On opening the abdomen I found an enlarged, generally diseased ovary, and a thickened and occluded tube buried in adhesions. No trace of the opposite tube and ovary. Diseased organs were thoroughly removed.

Convalescence afebrile. Patient was not relieved. She continued to menstruate irregularly, losing so much blood that she became quite reduced. Curettement was performed twice, without benefit. Uterus small, and contained no granulations. There remained a small induration at the site of the left stump, which was extremely tender, so that a year later I reopened the abdomen, excised the induration, which was merely an encysted ligature, breaking up slight intestinal adhesions. I found a small, atrophied uterus, which I ought to have removed, but did not. The patient remained

absolutely unrelieved, flowing profusely nearly all the time. She would have submitted to a fourth operation for extirpation of the uterus, but, unfortunately, acute intestinal obstruction occurred from which she died unrelieved.

CASE VIII. — This patient, a young married woman, who is still under my observation, was operated upon by the late Dr. Battey seven years ago. In a personal letter he stated positively that he had removed both ovaries and tubes, which were thoroughly degenerated. It is possible that a small portion of ovarian stroma remained in one of the ligatures. At any rate the patient has menstruated regularly ever since, though the flow has been scanty. The uterus is atrophied, as well as the vagina. In spite of her regular menstrual flow, the patient has had considerable local pain since the operation, and many of the nervous phenomena attending the climacteric, which have been more marked during the past year. Although she has never had children, the condition of the uterus is such as to destroy any prospects of maternity, even if this were desired.

These are simply a few cases which illustrate the unfavorable side of this question. In these cases, as well as in several others which I might cite, the element of pain and menstrual irregularity in the mind of the patient far outweighs the advantages accruing from persistence of the function, and possible hope of future maternity. This is an important practical point, because those who have written so strongly in favor of conservative operations have laid most stress on the importance of preserving the function of ovulation with the object of favoring conception.

In my experience, the first thing which women desire is to be relieved of the pain for which they have undergone the operation. When they do not find the promised relief, the question of conception becomes of secondary importance. In the case of young women who expect to be married, or of those who have been married only two or three years, this question assumes far more importance than with those who have been married for several years, or who have already borne one or more children. Here the principal object of the operation is, it is assumed, to relieve pain and to restore health. [Just after writing these lines the truth of this statement was emphasized by a case in which I sought to preserve a portion of a diseased ovary after removing the opposite one. The family physician who was present said: "Although the patient earnestly desires to have a child, unless you can promise positively that she will be cured I take the responsibility of urging that the ovary be entirely removed, as I am sure that she would prefer it."]

I would not be understood as deprecating the practice of conservative surgery; in fact, I have con-

sistently followed the plan in every case in which I thought portions of an organ could be saved without detriment to the patient, and have had satisfactory results. But I have merely sought to emphasize the fact that the immediate and remote results of conservative treatment are not always as brilliant as we have been led to believe.

In dealing with the question of sterility we are confronted with a problem which is by no means a purely mechanical one. In spite of the vast amount of literature on the subject, it being a favorite theme with French writers, we are still as far from understanding the occult reasons for this condition as we ever were; nor does it appear that we are yet in a position to give any more positive promises, as the result of our treatment by the abdominal route, than when we were limited to procedures for overcoming stenosis of the cervical canal. From our standpoint our chief motive in endeavoring to preserve the function of menstruation, in operations upon the pelvic organs, must still be the one insisted upon by Goodell, namely, to eliminate the mental and physical disturbances which attend the artificial climacteric in women who are still in the prime of life.

In this paper I have confined myself entirely to the subject of conservative surgery of the adnexa, believing that to discuss the question of myomectomy *versus* hysterectomy would carry me beyond the limits of this discussion. It is a fruitful subject and has been well handled in the Society time and again.

Personally, I am entirely in sympathy with every effort to preserve the uterus whenever this is possible. I am glad that we have not been entirely overwhelmed by the wave of enthusiasm which attended the visits of our distinguished French confrères, and that we are coming back to our former position, that the wholesale sacrifice of uteri is not in the direction of good surgery. But to enter into this question would be to renew an old fight. Much can be said *pro* and *con*. Doubtless we have all removed uteri which we might have saved, and, on the other hand, we have preserved organs which we were obliged to extirpate at a second operation. It is easier to criticise in the study than it is to decide correctly at the operating-table. Our esteemed Fellow, Dr. Baldy, in his usual epigrammatic style, sums up the whole matter when he says that "true conservatism is what is for the best interests of the patient."

In order to facilitate discussion the following conclusions are submitted:

1. Conservative operations on the adnexa are to be commended in properly selected cases. The surgeon should avoid, on the one hand, tampering with ovaries that are the seat of slight cystic degenera-

tion or cirrhosis, and, on the other, trying to preserve supposed normal tissue in organs which show such extensive disease that it is doubtful whether the best interests of the patient (both immediate and remote) would not be served by complete removal. In many cases it is advisable to simply separate adhesions. As there is no way of preventing their reformation, it is better to suture prolapsed tubes and ovaries at their normal level in the pelvis.

2. *Anatomical Results.*—In a certain proportion of cases resected ovaries undergo complete atrophy; in others the stromal remains may form the starting-point of cysts, requiring a second operation for their removal. A tube which has been rendered patent or resected, may again become occluded, or may form a hydrosalpinx or tubo-ovarian cyst.

3. *Symptomatic Results.*—These are often entirely satisfactory as regards the relief from pain and dysmenorrhea, the preservation of the functions of ovulation, and the occurrence of conception. Per contra, constant pain and dysmenorrhea may persist, menstruation may be absent, scanty, or excessive, and pregnancy is so far the exception that it to be regarded as an unusually fortunate sequence. In any case, we are not in a position to affirm how far conception following resection of the adnexa is directly due to this procedure, or how far to the accompanying treatment—curettement, separation of adhesions, restoration of the general health, improved sexual relations, etc. Our main object is the avoidance of the premature climacteric.

4. As regards technic, experience has shown that more successful conservative work can be done by the abdominal route, for reasons that are obvious, *i. e.*, thorough separation of adhesions, suture of raw surfaces, checking of hemorrhage, avoidance of drainage, etc. Catgut is preferable as a suture material.

5. As a corollary to the above, emphasis should be laid on the fact that, since the surgeon can never know the exact condition of the adnexa before opening the abdomen, he must not allow himself to be bound by any positive promise as to his course of procedure at the operating-table. While he should endeavor to preserve healthy organs and tissues whenever this is possible, and must necessarily be guided to a considerable extent by the expressed wishes of the patient, he must not be persuaded against his better judgment to practise conservatism at the immediate risk of her life or to court ultimate failure in order to give her the more than doubtful chance of future pregnancy.

The conscientious surgeon will not express himself too enthusiastically regarding the results of conservative operations for fear of arousing hopes which may be doomed to disappointment.

## THE CORRECTION OF SPINAL DEFORMITY BY STAGES UNDER AN ANESTHETIC.<sup>1</sup>

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THE presentation of a paper which must necessarily be incomplete is, of course, to be deprecated. The great interest in the subject, however, prompts me to make this preliminary report in order to record my own observations in the immediate correction of the deformity after Pott's disease, and also in that of lateral curvature of the spine. A brief paper on this subject was published by the writer in the *New York Medical Journal* in March of the present year. A few of the cases have already been placed on record in that journal, but as I have had the opportunity of observing the patients since (indeed, they are still under observation), I am simply here to-day to report progress. The exact records or tracings I shall not reproduce, as I have not removed the corsets or the jackets in all instances for this tracing, and because, too, the cases themselves are incomplete. It will be my effort to disabuse your mind, as my own mind has been disabused, of certain erroneous impressions and certain dangers which have been suggested by writers, some of whom are simply critics. At the same time I have no desire to give the impression that the operation itself is entirely free from danger. The patient, for instance, may take ether poorly, too much force may be employed at the first operation, the disease itself may be located in a dangerous part of the spinal column, or the personal element of the patient may be a contra-indication; yet I am convinced that with reasonable care, such as orthopedic surgeons are in the habit of employing in their work, the deformity may be corrected by repeating the procedure, say half a dozen times. Again, a certain amount of correction may be secured without the use of an anesthetic by frequent renewal of the plaster of Paris or whatever dressing may be employed. Especially is this desirable when one is not thoroughly conversant with the use of plaster of Paris. Even in the best of hands excoriations may sometimes occur, and excoriations occurring in cases like these under consideration necessarily bring discredit upon the treatment. We somehow feel that we are able to maintain a good position when once secured.

In England, where plaster has for a long time been tabooed, steel and iron appliances are employed, yet such appliances are generally managed or controlled by skilful men. On the Continent plaster of Paris, with an immense amount of cotton batting, is wrapped about the body, and it does

<sup>1</sup> Read before the American Orthopedic Association, Boston, May, 1898.



seem to me that the deformity is bound to recur where so much cotton is employed. The ideal dressing would, of course, be a skin-fitting plaster-of-Paris bandage secured accurately about the salient points, thus ensuring a perfect fit. In this country most of us are reasonably familiar with the use of plaster, and we take certain precautions that will guard against excoriation. It may be just as well to state that excoriations are not the worst things a patient can have. Even if a small ulcer is produced over the tip of the bosse, or a long one at the side of it, there is no real harm done. The only trouble is we are obliged to discontinue the fixation, during which period the deformity will recur. In my own cases I have not seen fit to include the head in the dressing. I have employed piano felting on either side of the bosse, over the iliac crests, along the free ribs where these protrude, and have bound these on with a cheese-cloth bandage after having secured these pads more firmly by means of a needle and thread. With such precautions I rarely ever find an excoriation, and want to put this on record, although I am unprepared, as above mentioned, to give final results so far as deformity goes.

In reapplying plaster when an anesthetic is not employed, I have been in the habit recently of hyperextending the column and the hips, thus securing a very decided recession of the deformity. At the time I began this work I was under the impression that the method had not been employed in the City of New York. I find since, however, that while this is true, it had been employed in Chicago. The cases are as follows:

CASE I.—A boy, twelve years of age, in very good general condition, but with a very awkward gait, presented deformity in the dorsolumbar region, resulting from old Pott's disease, many years having elapsed since acute symptoms prevailed. The tracing was taken before force was employed, and a very thorough examination was made in the iliac fossæ, in search of psoas abscess, with negative results. It was difficult, however, to completely extend the thighs. Under an anesthetic (gas and ether) I operated March 1, 1898, by making firm pressure over the bosse. I succeeded in breaking down the deformity, so that the tracing showed a straight line. There was an immense amount of crackling, the result of breaking up of heavy and ligamentous adhesions. On removing the pressure, however, there was a little recurrence, but on applying the jacket I felt quite sure that the good position would be maintained. Following instructions of Calot and others, I put the patient to bed, intending to keep him there for the next two or three weeks. The next morning he was ready to sit up, in fact, felt no pain whatever, and, strange to say, had passed a very good night without a narcotic. After three or four days he was allowed to get up,

there being no apparent reason for longer bed treatment. At the end of a fortnight we removed the jacket in order to reapply, without an anesthetic, and found a good-sized psoas abscess in the right iliac fossa. A large needle was inserted, but it was a dry tap. I applied plaster of Paris again, and a week or two later, under an anesthetic, employed more force, and have maintained this good position ever since. The abscess has given no trouble, and I doubt very much if it will. If it does I am prepared to take care of this complication.

CASE II.—A male, five years of age, deformity, Pott's disease, mid-dorsal. This boy has been in the hospital for a long time, had worn solid plaster-of-Paris jackets, and was quite accustomed to them. Examination made February 17, 1898, showed that his urine was normal; there was no pain or tenderness on pressure, and no sign of abscess. The disease was not at all active. On March 8th, under gas and ether, manual force was employed over the bosse, when the characteristic crackling was both felt and heard. While making pressure his respiration became affected a little, and the anesthetic was discontinued for one or two minutes; then, as danger seemed to have passed, it was renewed. The deformity in this case was not completely overcome. I should say it was reduced about two-thirds. He was put up in plaster of Paris, with the back in marked hyperextension. No unpleasant results followed. He was kept in bed for about two weeks; then on April 20, 1898, the jacket was cut down. There were no excoriations, except in one or two places, on the side of his body, where he had dropped some buttons. He was put in a swing at this time, and an attempt made to correct, with a fair amount of success. It has been rather difficult to keep his shoulders well back, although the plaster extended over them, and at the present writing I doubt very much whether the good position maintained at the time of the first operation remains. We expect to send him to the country for the summer, and in the fall the efforts at correction will be renewed.

CASE III.—A male, ten years of age, from the country, was admitted on April 5, 1898. There was incomplete paralysis of the lower extremities, but on the right side it was nearly complete. The reflexes were exaggerated, while there was no impairment of sensation. The deformity had existed, so far as the history was obtained, about two months only; the loss of power two or three weeks. The bosse was located in the upper dorsal. Two days after admission he was put under ether, and an attempt made to reduce the deformity, but it was only about one-half reduced. The plaster was carried well up around the neck and over the shoulders, and the dorsolumbar spine was put up in hyperextension. The following day there was a decided improvement in his limbs. He was able to move them about in bed quite freely, and he seemed anxious to stand up. This was not permitted, however, and special care was taken to prevent his sitting up. On the third day he had high temperature, and next morning it was found that he had developed scarlet fever.

He was taken at once to the Willard Parker Hospital, where he died within forty-eight hours. No autopsy was allowed.

CASE IV.—A girl, ten years of age, presented a deformity in the mid-dorsal region, the result of Pott's disease of long standing. There had been no acute symptoms for at least two years. On May 3, 1898, the spine was forcibly stretched and a plaster-of-Paris jacket applied. This child was rather nervous, and it was said that she had had for a long time, at irregular intervals, peculiar symptoms, such as spasm and pain, without any special cause. These attacks were thought to be hysterical, and left as suddenly as they had come on, never being followed by any other symptom. She was not allowed to get out of bed for three or four weeks. At this time the plaster of Paris was removed, further correction was made without an anesthetic, and this treatment will be continued until we secure a complete recession of the deformity.

CASE V.—A boy, six years of age, who gave a history of rickets. Deformity appeared some three years prior to this time, and he appeared at the Out-Patient Department of the hospital a year later. The deformity at that time was thought to be rachitic. On further examination at the time of his admission to the hospital, April 15, 1898, the deformity could not be corrected by manual force, and there was a history of pains pointing toward the disease being tubercular. He was put under an anesthetic April 20th, and direct pressure made over the bosse. The deformity was reduced at least one-half. Since then, without an anesthetic, it has been reduced still more, and he is at present under protective treatment.

These include all the cases of Pott's disease, while the following are cases of lateral curvature, in which, I am free to confess, results are far from brilliant. I am satisfied with the treatment thus far, yet it is a very slow process. One gains very little at each operation, and consequently gets discouraged. Inasmuch as the cases subjected to this procedure are extreme, as a rule, and inasmuch as the apparatus and exercises fail to effect any decidedly good results, I feel that it is our duty to resort to almost any method which is attended by the minimum amount of danger.

CASE VI.—The patient, an Italian girl, fourteen years of age, presented, at the time of her admission to the hospital in December, 1897, a very marked rotary lateral curvature in the dorsal region. This deformity had stunted her growth, and given her a typical hunchback, and it was undoubtedly as our English brethren say, "osseous." That is to say, we could affect no improvement by suspension. January 4, 1898, I attempted to stretch the back in an apparatus specially constructed for the purpose, employing one of the Schleich mixtures for anesthesia. The patient got too much chloroform and we had to desist. I question very much whether any reduction of the deformity was gained at this

time. In March I made two or three attempts under an anesthetic, and secured from two to three inches increase in height. I have been careful to maintain this improvement by renewing the jackets frequently while she was suspended in the swing. Certainly at this time there is some flexibility in the column, and I am encouraged to proceed with the treatment.

CASE VII.—A girl, six years of age, who presented a right rotary lateral curvature, rather rigid and unyielding. On March 22, 1898, the spine was forcibly stretched, and a fair amount of untwisting obtained. On March 24th she was again put under an anesthetic, and once or twice since then the plaster has been renewed. She has, since then, been wearing a solid plaster-of-Paris jacket, has not been suffering from excoriations, and there is a decided gain in height.

CASE VIII.—A girl, nine years of age, S curvature, rachitic in origin. She had been under treatment for a number of years at the hospital without material change. On March 21, 1898, under ether, I succeeded in reducing the deformity about one-half. She is wearing plaster of Paris now. The plaster has been renewed once or twice, and the treatment will be continued.

Such is my experience, and I intend to renew the work with vigor during the coming fall and winter. There has been a certain number of bad results in reported cases from the other side of the water, but none, I believe, on this side. It is well to bear in mind that any good thing can be abused. At the same time we should remember that the deformities which are now under discussion are most obstinate, and at times unyielding. The aim should be to break up these osseous adhesions and to render the column flexible. This being accomplished, and care being taken to hold the spine in better position for a few months, this treatment can be followed by gymnastic methods with greater hope of success.

#### FACTS REGARDING THE DEATH-RATE OF DIPHTHERIA WHEN BASED ON THE MORTALITY STATISTICS OF A CITY.

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To arrive at the truth in regard to the value of antitoxin as a curative agent in diphtheria when based on the mortality statistics of a city certain factors must be carefully considered: (1) One should be unbiased in his opinion regarding the value of a given remedy. (2) One should consider the factor that in all diseases there are certain years in which the effects seem to be more virulent than

in others, so that a series of years must be considered and compared. (3) In making the comparison one should go back to the time when bacteriologic methods were not used as a means of diagnosis. (4) A series of years must be compared both before and after the date of the introduction of antitoxin. (5) Errors in figures must be carefully noted.

Each week we read the mortality statistics of the larger cities, and often we are surprised to find that the death-rate of diphtheria, scarlet fever, and other infectious diseases appears exceedingly high, while at other times it seems relatively low. When it is low in diphtheria we judge that antitoxin is being more extensively used, and when it is high we wonder why it is so. Again, in studying the mortality statistics of hospitals we find this same condition existing, and although in some hospitals it is claimed that the antitoxin is being used and that the death-rate is not diminished over that of pre-antitoxin days, and we wonder why this is so.

The study of statistics is such dry reading that as a rule they receive but little attention. Generally we note the gross results, such as the average mortality, but when it comes to the "laryngeal cases," the "patients under one year old," the "patients requiring intubation," the "moribund patients" we begin to wander in a maze of a confusion of ideas and frequently these details are skipped. And yet these latter cases are very important. For instance, if we say that in pre-antitoxin days the death-rate of diphtheria was 29.62 per cent. (which it was in Philadelphia from 1892 to 1895 inclusive), and compare this result with that obtained in the treatment of "laryngeal cases," in "children under one year old," in "neglected or moribund cases" in which antitoxin is used, we may find that the death-rate from diphtheria in pre-antitoxin days was lower than that period in which this specific treatment was used.

Another fatal error is in comparing the death-rate occurring in years in which the mortality of nearly all diseases is low with those years in which the mortality of all infectious diseases seems relatively high. This is a great and favorite fallacy.

For instance, if we wish to prove the value of a certain remedy in the treatment of scarlet fever in Philadelphia for the year 1896, with that of 1894, it is found that in the year 1896 the death-rate from scarlet fever was 5.85 per cent. while in 1894 it was 13.63 per cent. Clearly there is yet no specific treatment for scarlet fever, yet in 1896 we find the death-rate less than half of what it was in 1894, the reason being that in 1896 nearly all diseases were milder in character than they were in 1894. As a

proof of this assertion reference may be made to the official report of the Bureau of Health:

## POPULATION OF PHILADELPHIA.

1892.	1893.	1894.	1895.	1896.	1897.
1,092,168	1,115,562	1,139,457	1,163,864	1,188,793	1,214,256

Death-rate of Philadelphia from all diseases from 1892 to 1897:

1892.	1893.	1894.	1895.	1896.	1897.
22.25	21.20	19.19	20.24	20.17	18.72

Number of cases of diphtheria, membranous croup, scarlet fever, and smallpox from 1892 to 1897:

	1892.	1893.	1894.	1895.	1896.	1897.
Diphtheria.....	5051	3149	3159	3351	3191	5031
Membranous Croup	not obtainable	322	449	502	404	374
Scarlet Fever.....	6350	2849	1122	1195	1042	3553
Typhoid Fever.....	2304	2519	2357	2748	2489	2994
Smallpox.....	7	43	136	319	none	none

Number of deaths from diphtheria, membranous croup, scarlet fever, typhoid fever, and smallpox from 1892 to 1897:

	1892.	1893.	1894.	1895.	1896.	1897.
Diphtheria.....	1435	916	1047	1020	862	1231
Membranous Croup	Reported as diphtheria	243	349	329	293	243
Scarlet Fever.....	485	267	153	79	61	282
Typhoid Fever.....	440	456	370	469	402	401
Smallpox.....	none	5	13	45	none	none

Mortality from diphtheria, membranous croup, scarlet fever, typhoid fever, and smallpox from 1892 to 1897:

	1892.	1893.	1894.	1895.	1896.	1897.
Diphtheria.....	25.57	29.08	33.11	30.75	27.01	24.48
Membranous Croup	Unattainable	75.46	71.71	65.53	72.52	64.97
Scarlet Fever.....	7.63	9.37	13.63	6.61	5.85	7.93
Typhoid Fever.....	19.09	17.75	15.69	17.06	16.15	13.39
Smallpox.....	none	11.62	9.56	14.10	none	none

It will be observed that the death-rate of Philadelphia is gradually being reduced, and it may be added that the number of deaths from at least two common diseases has shown an improvement as shown by the following statistics:

Deaths from phthisis from 1892 to 1897; also, the death-rate per 1000 persons living:

	Deaths.	Population.	Deaths per 1000 persons living.
1892....	2709	1,092,168	2.480
1893....	2671	1,115,562	2.394
1894....	2513	1,139,457	2.205
1895....	2449	1,163,864	2.104
1896....	2514	1,188,793	2.115
1897....	2388	1,214,256	1.966

(These figures seem to refute the statements frequently made that death from phthisis is becoming more common in cities.)

Deaths from diphtheria from 1892 to 1897; also, the death-rate per 1000 persons living:

	Deaths.	Population.	Deaths per 1000 persons living.
1892....	1435	See above	1.322
1893....	916	" "	.821
1894....	1047	" "	.902
1895....	1020	" "	.876
1896....	862	" "	.771
1897....	1231	" "	1.013



In making comparisons of the mortality of diphtheria at the present time when the culture method is in vogue with that of pre-antitoxin days, we must allow for mistakes which formerly resulted when the clinical diagnosis was mainly relied upon. It might, therefore, be of interest to compare the results of the two methods of diagnosis as obtained in Philadelphia. In the report of the Bacteriologic Division of the Bureau of Health for 1895, pages 232-233, we find the following statistics on this subject based on 1207 cases:

Table A, showing the results of all cases and the relation of same to the clinical diagnosis, 1207 cases.

Diphtheria bacilli present.		No diphtheria bacilli present.		Unsatisfactory.		Clinical Diagnosis Given.		Clinical Diagnosis of Diphtheria.		Clinical Diagnosis not Diphtheria.		No Clinical Diagnosis and doubtful, per cent.	
				Cases.		Verified, per cent.				Cases.		Verified, per cent.	
775	291	141	705	86.4	557	90.2	148	72.9	41.5				

It will, therefore, be observed that the percentage of correct diagnoses made by clinical methods in Philadelphia, and subsequently confirmed by a bacteriological examination, is relatively high, being verified in 90.2 per cent. of the cases, but shows a loss when the disease was not properly diagnosed.

The culture method as a means of diagnosis was instituted in Philadelphia on May 20, 1895. Membranous croup is gradually disappearing while the percentage of cases of diphtheria shows an increase. Many argue that since the culture method has been in vogue that cases are now recognized as diphtheria, which were formerly not suspected on account of their mild character, and that a relatively large percentage of these patients would recover even though antitoxin was not used. Undoubtedly this is partly true but it evidently does not hold good for the year of 1896. It might be of interest, therefore, to give this increased percentage of cases as reported to the Bureau of Health.

Number of cases of diphtheria from 1892 to 1897, with the rate per 1000 persons living:

Cases.	Population.	Cases reported per 1000 persons living.
1892....	5051	1,092,168
1893....	3149	1,115,562
1894....	3159	1,139,457
1895....	3351	1,163,864
1896....	3191	1,188,793
1897....	5031	1,214,250

The writer endeavored to determine approximately the number of practitioners who availed themselves of

the opportunity of having a bacteriological examination made as a means of diagnosis in suspected cases of diphtheria. It is stated in the "Annual Report of the Bureau of Health" that since January, 1897, the routine work of the laboratory has consisted in the bacteriologic examination of cultures from cases of diphtheria and suspicious inflammations of the throat and nose. For the year 1897 there were 3353 persons suspected of having diphtheria, and of these 2020 proved by bacteriological examination to have diphtheria bacilli, 1138 did not have diphtheria bacilli, and in 195 cases it was impossible to make a positive diagnosis. As there were reported to the Bureau of Health for 1897 a total of 5031 cases of diphtheria, it will be observed that about 66 per cent. of the profession availed themselves of this method of diagnosis at the hands of the Bureau of Health. It should likewise be noted that of the 3351 cases in which a bacteriological examination was made 33 per cent. of them did not show the presence of diphtheria bacilli, and these doubtless would have formerly been reported as cases of diphtheria and treated as such. Doubtless if we should take a series of years we might find that the percentage of mistakes that formerly occurred from a clinical diagnosis would not be far from that increased rate of reported cases as now observed when dependent upon the culture method as a means of diagnosis.

In the American Pediatric Society's "Report on the Collective Investigation of the Antitoxin Treatment of Diphtheria in Private Practice" (MEDICAL NEWS, May 15, 1897), it was stated that the mortality in 1704 laryngeal cases (worst type of the disease) in which antitoxin was used was 21.12 per cent., and in the 668 patients operated upon the mortality was 27.24 per cent. In nearly 6000 cases in which the antitoxin was used the first day the mortality was 4.9 per cent., for the second day 7.4 per cent., for the third day 8.8 per cent., for the fourth day 20.7 per cent., and for the fifth day 35.3 per cent. A careful study of the mortality statistics of Philadelphia evidently shows one of three things: (1) The period is yet too early to base an absolute conclusion regarding the beneficial effects of antitoxin in diphtheria. (2) That antitoxin is not used as frequently as it should be; or (3) its administration is postponed to a period in the disease when the chances of recovery on the part of the patient has become gradually lessened.

*Professor Koch in Rome.*—It is reported that Professor Koch has already arrived in Rome for the purpose of making a searching investigation into the etiology and nature of malaria, and the methods of infection. With that object in view every facility has been placed at his disposal by the municipal and collegiate authorities.

**SEPTIC BRONCHOPNEUMONIA.**

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IN the conventional nomenclature of pulmonary diseases, a tuberculous and a simple bronchopneumonia are recognized. In both affections the essential anatomic condition is an accumulation of inflammatory products in the smaller bronchi and their alveolar continuations, environed by hepatized tissues. The clinical differentiation between the two forms is in the presence of tubercle bacilli in the sputum. In the adult, bronchopneumonia is capable of further division from the standpoint of the clinician. During the past ten years, I have kept careful notes of all cases of bronchopneumonia, 61 in number, which have come under my observation. In accordance with my observations I am justified in adding to the list two other forms of the disease, and have denominated them, (1) non-febrile, chronic bronchopneumonia; and (2) septic bronchopneumonia. The cases which came under observation may be divided into tuberculous bronchopneumonia, 25 cases; simple bronchopneumonia (the conventional form), 10 cases; non-febrile, chronic bronchopneumonia, 15 cases; and septic bronchopneumonia, 11 cases. The last two forms are the subject of consideration in this contribution.

Excluding the tuberculous form from discussion, it is customary to regard bronchopneumonia occurring in adults as a foudroyant affection, attended by pronounced dyspnea, cyanosis, elevation of temperature, and symptoms of collapse. In the non-febrile form of bronchopneumonia, 15 of my cases admit of analysis as follows: course, subacute in 4, and chronic in 11 cases; no elevation of temperature in any one of the cases; slight dyspnea in 5 cases. Expectoration in nearly all the cases showed the presence of staphylococci and streptococci. The physical signs were those of bronchitis confined to the smaller tubes, percussion-note dull over discrete lung areas, notably the right lower lobe and the apices, with bronchovesicular, rarely bronchial respiration.

In the septic form of bronchopneumonia (11 cases) the clinical picture was essentially that of pulmonary tuberculosis, minus the presence of tubercle bacilli in the sputum. Such cases in the prebacillary period of medicine would have been regarded as indubitable cases of phthisis; in fact eight of the patients came to me with that diagnosis. The symptoms of these cases were invariably those of sepsis—remittent and intermittent types of temperature, night-sweats, chills, pronounced and rapid emaciation, anorexia, etc. In the majority of the pa-

tients the site of disturbance was limited to the pulmonary apices. The sputum of these patients was of a purulent character and contained in most instances streptococci and staphylococci, coli communis, and pneumococci in large numbers. The prognosis in both forms just described was good, cure being secured in the majority of cases by the pneumatic cabinet. The same results might have been attained with compressed air by means of any other efficient apparatus. I lay stress upon the employment of compressed air, and advisedly, because nearly all the patients who underwent treatment had resisted the conventional therapeutic methods. I mention the pneumatic cabinet particularly, because I believe it embodies the most rational means yet devised for the administration of compressed air. The effect of compressed air in these cases virtually secured a mechanical dislodgment of the inflammatory and septic products in the smaller bronchi and their alveolar continuations. Free drainage of the septic districts is obtained by aerial lavage. If this mechanical treatment is procrastinated there is great danger of the exudate in the non-febrile forms becoming septic, and of the septic form becoming tuberculous.

In addition to this treatment, iodid of potash was employed, this drug being one of the most efficient expectorants we possess. Its liquefactive action on the exudate is almost marvelous.

The following conclusions may be formulated:

1. Bronchopneumonia may exist as a chronic non-febrile affection.
2. Septic bronchopneumonia may simulate pulmonary tuberculosis.
3. The only clinical distinction between septic bronchopneumonia and septic pulmonary tuberculosis is in the presence of the tubercle bacilli in the latter affection.
4. The specific treatment of septic bronchopneumonia consists in free drainage by aerial lavage of the septic districts by compressed air and the employment of iodid of potash to facilitate expectoration.

**CLINICAL MEMORANDA.****CARCINOMATOUS THROMBOSIS OF THE BRACHIAL ARTERY.**

By F. R. FRAZIER, M.D.,  
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MRS. J., ninety-four years of age, was first seen September 24, 1897, at which time she presented an indurated mass, about the size of a hickory-nut, located on the margin of the lower lip to the left of the median line. A diagnosis of carcinoma was made, and an operation deemed inadvisable on account of the patients' advanced age, her

feeble condition, and extensive glandular involvement. She was next seen on March 2, 1898. The lower lip was nearly destroyed. There was very extensive cervical and mediastinal glandular involvement. A suppurating fistula existed on both sides of the larynx. Two days previously the patient had experienced sudden and acute pain in the right arm. The hand and forearm were noticed to become extremely pale and cold. When seen, the hand and forearm were discolored, cold, and anesthetic, while the fingers showed a very marked tendency to dry gangrene. Her condition was bad. She was semicomatose, and had Cheyne-Stokes' respiration. A dry antiseptic dressing was applied. She died eight days later without any evidence of sepsis. Dry gangrene of the hand and forearm was present. No autopsy was permitted.

### CYST OF THE THYRO-HYOID BURSA.

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MISS B., nineteen years old, had had for about ten years a small swelling in the median line of the neck which had occasionally discharged through a pin-hole opening, a glairy mucus-like fluid. This swelling has varied in size from a pea to a hickory-nut. At times it has grown smaller from no apparent cause. Whenever the patient has had a cold this swelling has always become tense and full.

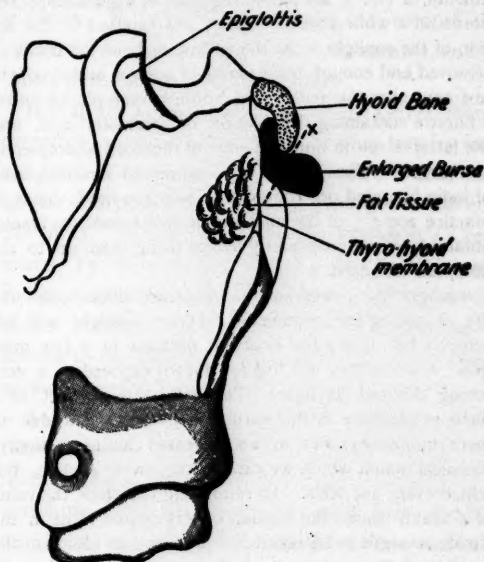
Examination revealed at the angle of the neck with the chin, just above the level of the top of the thyroid cartilage, exactly in the median line, a tumor, which was firm, elastic, rounded, the size of a hickory-nut. This tumor was attached to the deeper parts and moved with deglutition. The overlying skin was adherent in one spot where a small fistulous opening existed, admitting a fine surgical needle.

Three operations were performed upon this small tumor. The first was done in November, 1893, under cocain anesthesia. The tumor was exposed by a small transverse incision and the sac of the tumor was opened. The glairy mucus contents were evacuated. A fine probe passed downward and upward behind the body of the hyoid bone. The sac was removed down to the level of the hyoid bone and beyond this point was stimulated by the application of strong carbolic acid. The application of carbolic acid was made with the hope that the remainder of the sac might granulate and heal. The wound was closed and healed promptly. The tumor did not reappear for about six weeks but then became very evident.

On May 29, 1894, a second operation was done, again under cocain anesthesia. At this time the part behind the hyoid, inaccessible to the knife without dividing the hyoid body, was most thoroughly cauterized. The tumor again reappeared after a few weeks.

A third operation was performed on October 30, 1895, under ether anesthesia. A transverse incision was made over the swelling and the newly formed cyst sac isolated

down to the body of the hyoid. There was no trace of the sac's running up behind the hyoid. This had been completely obliterated. The parts about the base of the little sac were carefully examined and no opening could be found. The wound was sutured securely without drainage by deep and superficial sutures.



Case of thyro-hyoid cyst. (In this illustration the right half of the hyoid bone and thyroid cartilage have been removed.) The bursa was excised at the point indicated by the dotted line.

Up to the present time there has been no return of the swelling. The accompanying diagram shows very well the relations of the parts involved in the cyst surroundings. Had the last operation not been successful I intended splitting the body of the hyoid bone, retracting the two halves and dissecting out the sac of the bursa, thus preventing any possible recurrence.

### MEDICAL PROGRESS.

*The Measurement of Sunshine at Health-Resorts.*—JONES (*Lancet*, July 30, 1898) has been measuring the actinic value of the sunshine, in summer and winter, in London and at Llangammarch Wells, a mountain-resort in Wales, where the air is particularly pure and clear. He found that the actinic value of the sunshine in the latter place was in an hour of a summer's day from three to five times that of London under similar conditions of clear or clouded skies. In the winter the difference was not so marked, but the Wales sunlight was about twice as strong as that of London. A comparison with the results of similar tests made in the high Alps shows that the air may be just as pure in a mountainous district or far less lofty elevation. These tests are essentially a test of the purity of the air, as the amount of heat in the sun's rays has no effect upon the result. The method employed is briefly as follows: A solution of potassium iodid is prepared



containing 20 grams to the liter of water; also, a solution of pure sulphuric acid, 11.6 grams to the liter of water; and a third solution in which a liter of water contains 0.39 grams of powdered arsenious acid and 1.5 grams of potassium bicarbonate. To make a test of the sunlight 10 c.c. of solution No. 1, and an equal amount of No. 2, are placed together in a glass-stoppered bottle on a white porcelain plate, and exposed to the action of the sunlight. At the end of an hour the bottle is removed and enough bicarbonate of soda is added to it to just neutralize the acid. The bottle is then placed under a burette containing the solution of arsenious acid, and the latter is run in until the color of the iodine is completely discharged. The results are expressed in milligrams of iodine liberated per 1000 c.c. of solution used. Since in practice 200 c.c. of the mixed solution are used, the results obtained were multiplied by five to bring them up to the accepted standard.

Sunlight has a well-known inhibitory effect upon the life of pathogenic organisms. Direct sunlight will kill tubercle bacilli in a few hours or perhaps in a few minutes; whereas they will live for days if exposed to a very strong diffused daylight. The exhilarating effect of a burst of sunshine in the spring is probably not due to mere luminosity, but to an increased actinic action, a chemical action which we cannot very well explain, but which every one feels. In estimating therefore the value of a health-resort, the amount of this actinic value in the sunshine ought to be taken into account, no less than the number of days upon which the sun shines during the month or the year.

**The Diagnosis of Frontal Empyema.**—GOULY (*Med. Moderne*, May 7, 1898) says that empyema of the frontal sinuses is frequently overlooked. In typical cases three cardinal symptoms exist: violent pains at the root of the nose; nasal obstruction, and a discharge of pus from the nose or through the nasopharynx. The pain may be dull and continuous; or it may be paroxysmal with its seat in the region of the frontal sinus, or disseminated over the whole head. At times it is extremely violent, especially if the secretions can not escape from the frontal openings. The pain may be intermittent. It can be aroused by percussion on the base of the forehead.

At a more advanced stage of the affection, the skin overlying this region becomes infiltrated and edematous or there may be a hyperostosis of the external table of bone. The orbit itself is affected, and edema of its upper portion and a deviation of the globe of the eye may be noticed. Rhinoscopy will show that the openings into the frontal sinus are closed, usually by mucous polyps. The character of the pus is significant. In suppuration of the maxillary sinus the pus is almost always grumous and forms a caseous mass which does not readily flow away. In suppuration in the frontal sinus it is thick, creamy, and not fetid.

Lighting the sinus by electricity will corroborate the results of direct examination, but is not of itself sufficiently conclusive to make a diagnosis positive, especially if its results are opposed to those of other methods of examination. Both sides should be lighted and compared, in or-

der that any difference may be manifest. Both sides are rarely affected simultaneously, as that presupposes that rhinitis and obstruction have been bilateral, which is not usually the case.

**A Cancerous Family.**—POWER (*Brit. Med. Jour.*, July 16, 1898) gives what is perhaps the most remarkable family predisposition to cancer ever recorded of the members of any family. A patient in the Infirmary at Peterborough had his right breast removed for scirrhus cancer in 1896. The following year malignant glands were removed from the axilla. He returned to the hospital this year with a recurrence in the axilla, too extensive to allow of removal. His father died at the age of 46 years of cancer of the breast. His mother died at the age of 86 years of no particular disease. He had two brothers and eight sisters. His first brother died at the age of 65 years of cancer of the throat. His second brother died at the age of 24 years of cancer of the left axilla, the breast never apparently having been affected. Of his eight sisters, the first four died of cancer of the breast, at the respective ages of 63, 46, 40, and 54 years; the second and third sisters having the disease on both sides. The fifth and sixth sisters were alive at the time of the report, but both of them were suffering from cancer of the breast. The seventh sister died while yet a child, and the eighth sister died in childhood.

## THERAPEUTIC NOTES.

**Aspiration of the Contents of the Stomach During Artificial Respiration.**—BROSCH (*Therap. Monatshefte*, July, 1898) was led by an unfortunate death during the performance of artificial respiration to make an investigation of the possibility of the aspiration of material from the stomach under these circumstances. He found by thirty-one experiments made upon the cadaver that there is a strong aspirating power when artificial respiration is performed. This force is equal to the aspiration of from 1000 to 1210 centimeters of fluid (more than two pints). The latter is the maximum amount, the aspiration of which he was able to accomplish. It follows that it is impossible to remove fluids and foreign bodies from the middle and lower portions of the lungs, excepting, of course, by operative measures. The conditions for the aspiration of a large quantity of fluid during artificial respiration are more favorable in the living than they are in the cadaver. Pressure below the diaphragm is an especially dangerous procedure, as it was shown that such pressure can easily force fluids from a full stomach into the throat, the cardiac orifice, even in the living, affording little protection against such a regurgitation.

How may such a regurgitation and aspiration be guarded against? Of all the measures proposed and tested only one was found to have an absolute value, and that was the introduction of a tube into the esophagus before the movements of artificial respiration were begun. The procedure is simple enough, and it did not once fail of its end. The author, therefore, recommends that it be performed in all cases before artificial respiration is begun.

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SATURDAY, SEPTEMBER 24, 1898.

## THE ARMY INVESTIGATION.

THE determination of President McKinley to appoint a committee and order an investigation of the alleged neglect, incompetency, and abuses in the management of army affairs gave foundation for the hope that the facts concerning the War Department's conduct of the war would be brought clearly before the public and the responsibility for mistakes and incompetency definitely fixed. It would seem that the President entered upon this undertaking with all sincerity, but evidently without due consideration. The President can order an investigation and appoint a committee, but, unfortunately, no committee thus appointed can be adequately empowered, except by act of Congress, to prosecute an investigation efficiently. It cannot judicially administer an oath, compel the production of papers, or the attendance of witnesses, nor has it the power to punish indifference to its behests. In each House of Congress, however, inheres the power to conduct adequately such investigations as the present contingency requires, according to the Revised Statutes:

101. The President of the Senate, the Speaker

of the House of Representatives, or a chairman of a Committee of the Whole, or of any committee of either House of Congress, is empowered to administer oaths to witnesses in any case under their examination.

To them is also given the power to summon witnesses, to insist upon answers, to compel the production of pertinent papers or documents, and to inflict punishment for recalcitrancy.

It is to the legislative branch of the Government, therefore, we must look for a searching inquiry into the conduct of the war in all its phases. This cannot be undertaken till Congress convenes in December next. In the meantime the President's Committee by devoting its inquiry to the volunteer camps may be able to throw some light upon the causes of their deplorable condition, and so be of service to the general betterment of the army camps as well as to the saving of many lives.

As the revelations from different quarters group themselves and the details of suffering, neglect, and incompetency become known the necessity of a searching investigation asserts itself with steadily increasing insistence. The Surgeon-General of the army has been charged with dereliction in not having had a sufficient supply of ambulances and medical and surgical stores during the Santiago campaign. In reply to this the Surgeon-General says that an abundant supply of everything needed in his department was ordered and would have been at hand had it not been for the utter failure of the Quartermaster's Department, which was responsible for the transportation. His charges have corroborative evidence in the facts that have been steadily coming to light indicative of the entire lack of system in loading the transports, and absolute ignorance of the requirements of such an undertaking. No one can be held responsible for not having had experience in such matters, but the least that can be asked of one who assumes this responsibility is that his experience shall have been such as to develop the faculties necessary to conduct work of that character with judgment, and that he shall have a reasonable acquaintance with the recorded experience of others. In Field-Marshal Lord Roberts celebrated book, "Forty-one Years in India," vol. 2, p. 28, in connection with the Abyssinian campaign we find recorded the following simple directions for loading transports:

I had often read and heard of the difficulties and delays experienced by troops landing in a foreign country in consequence of their requirements not being all shipped in the same vessel with themselves—men in one ship, camp equipage in another, transport and field-hospital in a third, or perhaps mules in one and their pack-saddles in another; and I determined to try and prevent these mistakes upon this occasion. With Stewart's permission I arranged that each detachment should embark complete in every detail; which resulted in the troops being landed and marched off without the least delay as each vessel reached its destination.

This precedent, of so simple a principle so successfully applied, if followed in the shipment of the Fifth Army Corps to Cuba would have provided the invading army with field- and siege-guns, commissary stores, ambulances, and medical and surgical supplies. Instead of which, as a matter of fact, the guns were packed in one ship, their carriages in another and their wheels in a third. The various parts of the ambulances were distributed throughout the fleet, and the medical supplies after primarily being entirely left behind at Tampa, were later stowed away in some ship and carried back and forth before they were finally landed. And for this the medical arm of the service is held responsible!

It has been suggested that the most judicious thing the President can do now is to abandon his project for an investigation and await the action of Congress, as the result of the work of his committee could not fail to be unsatisfactory to the country. In the management of the volunteer camps, however, there is a field for the inquiries of this committee provided they can be promptly undertaken and promptly concluded, for the purpose of informing the President how these camps have been managed and under whose supervision they have been placed. To mention one instance, they would have no difficulty in discovering that one of the most responsible medical positions in the entire army has been filled, not by the promotion of the best medical officer in the regular army—a man of medical and surgical experience as well as familiarity with camp-life and army methods—nor by a prominent physician taken from civil life who had a national reputation as a sanitarian, but by an obscure veterinary surgeon. The position referred to is no less than the chiefsurgeonship of an entire army corps. This veterinary surgeon has been the chief medical officer in charge of Camp Thomas at Chickamauga Park. His appointment was not made at the

recommendation of the Surgeon-General of the army but as a result of political influence. Every medical staff officer in this entire army corps had to submit to the mortification of being *visé* and approved by this same veterinary surgeon. The responsibility for the sanitary arrangements and conditions of an encampment containing 40,000 men was vested in a man who, probably, never in his life gave one moment of serious reflection to the personal hygiene or sanitation of a body of men however small; a veterinary surgeon whose entire medical experience for the past fifteen years has consisted in doctoring horses, cattle, dogs, and cats. What else could be expected in a camp entrusted to the supervision of such a man than that it should have a reputation for filth and uncleanness; that the water should be contaminated, and that the ranks should be ravaged by a frightful epidemic of typhoid fever.

#### THE CAMPS OF INSTRUCTION.

THE time has now come when we can dispassionately and impartially review the medical aspect of the military campaign now closing. We can clearly ascertain the causes of the misfortunes which have befallen the army and determine the responsibility for them. If we shall seem too severe in our strictures it is in deep humiliation that we are forced to record the shortcoming of those for whom, as members of our profession, we are disposed to find excuse; and we fall far short of painting an exaggerated picture.

The camps of instruction were for the purpose of familiarizing the volunteer with the duties of the soldier, train him for active service in the field, and particularly to bring him to a high physical efficiency. At the first call of the President each State entered into generous rivalry to furnish its quota of troops. Recruiting to the war standard was vigorously prosecuted, and mobilization was rapidly effected. Here we find the first cause of the inordinate amount of illness which has rendered the army practically useless. Adjutant-generals, owing their appointments to political favoritism or personal friendship, without military experience and devoid of even common sense, have imagined that the gathering together of masses of men in a hastily determined upon rendezvous was "mobilization" in the proper acceptance of the term. Thus it came about that a horde of



ununiformed, unequipped men, with some companies of the National Guard were sent into camp. While it is no particular hardship for a properly equipped soldier to bivouac in the inclement weather of our spring it was positive brutality, born of selfish conceit, to send unsuitably clad civilians to a camp which was unprepared and without sufficient tentage. An adjutant-general, so ignorant of the duties of his office that he assumed command of a portion of the National Guard while a ranking officer was present could be expected to do no better. The recruits should have been retained in the armories until such time as they could be properly equipped. The time might have been profitably employed in setting-up drills and instruction. These men were but obstacles to camp routine and that they were not incapacitated by unnecessary exposure was due to good fortune rather than to good management.

With the ignorance and incompetence of adjutant-generals begins the chapter of mismanagement which has wrecked as fine a body of men as ever assembled at the call of a nation's ruler. With the assigned reason that raw troops would be more quickly fitted for service in the tropics most of the camps were located south of Mason and Dixon's line. Facility of transportation to the supposed port of embarkation was also considered. It is claimed that not all camp-sites were passed upon by competent medical authority. It is to be hoped that this is true, for the surgeon who would certify that a suitable camp-ground is found where solid rock outcrops within three feet of the surface, so that the sinks could never be properly covered, certainly showed ignorance of one of the fundamental principles of camp hygiene. Nor indeed could a site be chosen upon the recommendation of a surgeon when the water-supply, polluted by one portion of the camp, was to be used by another section. The location of sinks, the disposal of refuse, and the proper policing of camps are matters for the exercise of common sense and good judgment.

An effort has been made to account for the acknowledged failure of the Medical Department by placing the blame upon the volunteer and acting assistant surgeons. To give point to the charge of incompetency and inexperience the latter are called "contract surgeons." To our mind there is no essential difference between a "contract," volunteer,

or army surgeon. Many of the acting assistant and volunteer surgeons are men of greater executive ability, larger experience, and higher professional standing than their fellows in the army.

The difficulty in transporting supplies added to unhygienic camp discipline is responsible for the outbreak of fevers so extensive that every regiment upon the march left its trail of disease. That the surgeons alone, no matter how experienced or competent, could have kept a respectable sick percentage, is evident from a single instance coming under our observation. A permanent state camp occupied by a regiment of infantry and a battery of artillery after two months became so dirty that the odor of the sinks and of the kitchens was perceptible in almost any portion of the camp. It is said that there were seventy prisoners in the guardhouse, yet the camp was not properly policed. The men, in spite of two-months' drill, were not well set up and none showed the alertness and snap of the soldier. With a commanding officer who would be satisfied with such slovenly conditions the surgeon, whom we know to be competent and efficient, was powerless to maintain a respectable standard of health. In fact, at the time of inspection, an epidemic of typhoid fever had begun, and later extended so that the projected encampment of a home regiment became impossible. This was a camp of small size, near to markets, well equipped, containing men of far above the average intelligence, which became infected simply because it was not kept clean. What occurred in this instance undoubtedly came to pass in most of the camps. The causes of disease in the camps of instruction have been Falstaffian adjutant-generals, camps located for reasons other than military or hygienic, and finally camps dirty beyond the power of the surgeons to control. The Secretary of War has sought to offer excuse for the abuses existing by stating that in the work of creating an army of two hundred thousand men great difficulties were encountered. Had the Secretary created such an army some slender excuse might be accepted. The facts are that some two hundred thousand men, chosen for their good health and strong physique, are taken from their homes to be trained in the discipline of soldiers. Without having faced an enemy, without leaving the country, a regiment returns two hundred and sixty sick and marches through the streets of

the metropolis, scarcely a battalion fit for duty. The Secretary has created an army, but it is an army of the sick and dying.

## ECHOES AND NEWS.

**No Yellow Fever in Porto Rico.**—Despatches from Ponce, dated September 19th, report that there are no more suspicious cases of yellow fever developing, and that the alarm incident to the early reports has entirely subsided.

**Yellow Fever in Louisiana.**—The Louisiana State Board of Health reported September 19th three additional cases of yellow fever in New Orleans, making four in all. No deaths. Also five cases in an Italian camp at Harvey's Canal, in Jefferson Parish.

**Bellevue Appointments.**—Commissioner Keller of the Department of Charities has appointed to the visiting staff of Bellevue Hospital, Dr. Henry C. Coe, gynecologist; Dr. L. Bolton Bangs, genito-urinary surgeon; Dr. B. Farquhar Curtis, surgeon; Dr. George D. Stewart, surgeon, and Dr. Egbert LeFevre, physician.

**Yellow Fever in Mississippi.**—The Secretary of the State Board of Health of Mississippi reported September 13th, "One case of yellow fever in Jackson. Already steps have been taken to stamp out and prevent the spread of the disease." No new cases have been reported from Franklin, Norwood, Holly Springs, Galveston, or Key West.]

**Married Women as Public-School Teachers.**—The Board of Education of the Borough of Queens, New York City, at its recent session adopted resolutions to the effect that when a teacher gets married it will be considered equivalent to her resignation. It was also decided that hereafter the application of married women for positions as teachers must be endorsed by the unusual number of five members of the Board.

**The Health of New York City.**—In the report of the Health Department for the week ending September 17, 1898, appear the record of 169 new cases of tuberculosis and 145 deaths; typhoid fever, 209 new cases and 44 deaths; diphtheria, 102 new cases and 18 deaths. The increase in the number of typhoid cases is apparently due to the reception of soldiers into the hospitals upon their return from Montauk and the Southern camps.

**Mortality among the Spanish Soldiers Returning to Spain.**—The transport "San Ignacio" sailed recently from Santiago with 1000 Spanish prisoners. Of these, 123 died on the voyage. The high mortality among the troops returning in transports to Spain is ascribed by the Spaniards to the inhumanity of the Americans in compelling men in a critical condition to embark so that the Santiago hospitals could be cleared for the American sick.

**The Transport "Vigilancia" Arrives at Montauk.**—The transport "Vigilancia" which left Santiago on September 6th arrived at Montauk Point September 13th. She

brought 320 passengers of whom 76 were sick and 15 ailing. Among those on board were 22 nurses, most of them colored and immune. This ship brought the last detachment of the Fifth Army Corps. Thirty sick soldiers and 8 nurses are all that remains of the army in Cuba.

**The Fondness of Soldiers for Pie.**—The soldiers of the Volunteer camps have developed an extraordinary fondness for pie particularly when recovering from typhoid fever, dysentery, or some other ailment which requires the greatest care in diet. Various devices have been resorted to to prevent them from gratifying their fatal longing, and recently the authorities at Camp Wikoff promulgated an edict forbidding the sale to soldiers of pies, pastry, or ginger ale.

**The Louisiana State Board of Health.**—In accordance with the law enacted by the last legislature requiring the Governor of the State to appoint seven representative physicians to constitute the State Board of Health, the following gentlemen have been selected and appointed: Drs. Edw. Souchon, President, Orleans Parish; H. S. Lewis, Orleans Parish; Chas. Goudel, Orleans Parish; J. P. Egan, Caddo Parish; W. G. Owen, Iberville Parish; R. L. Randolph, Rapides Parish; T. T. Tarleton, St. Landry Parish.

**The Chicago Society of Internal Medicine.**—A joint meeting of this Society and the Chicago Medical Society was held Wednesday evening, September 21st. Dr. John A. Robinson delivered a Presidential address. Dr. Daniel R. Brower, in an elaborate paper, opened the discussion upon "Acute Cerebral Meningitis," and Dr. Adolph Gehrmann read a paper on "The Results of Widal's Test in Typhoid Fever." The annual meeting will be held in January. The subject for discussion will be "Acute Articular Rheumatism."

**The New Building of the Jefferson Medical College, Philadelphia.**—Final plans for the new building of this institution have been approved by the trustees. The structure when completed will be one of the handsomest of the kind in this country and will cost about \$250,000. The contract calls for the completion of the building by June 1, 1899. The building will contain the lecture-rooms, various laboratories, dissecting-rooms, etc. A unique feature will be the provision made for a gymnasium, reading- and billiard-rooms, and bicycle-lockers.

**Typhoid Fever at Camp Meade.**—The latest reports from Camp Meade indicate the presence there of more than one hundred typhoid-fever patients in the division hospitals. The sudden increase in the number of typhoid cases is attributed to the arrival of two New York regiments from Camp Black. The cases that developed at Camp Black in these regiments were of a mild type, but it is thought that the effort of breaking camp, making the long journey, and the change of location has changed the character of the disease to its worst form.

**The Tax on Medicine Questioned.**—A controversy has arisen in the Internal Revenue Bureau regarding the

proper classification of euophen, phenacetin, acetylene, lysitol, celoplien, pyretin, also sulfonal, trional, etc. The manufacturers of these drugs failed to place the proper revenue stamp upon them, claiming exemption as non-compounded articles. A test case is to be made as to the liability of these drugs to taxation, and the Collector at New York recently seized the place of business of an agent for these preparations. All the articles in question are to be stamped pending the decision of the court.

**The Low Birth-Rate of Ontario, Canada.**—The Montreal Anglican Synod recently held a discussion upon the low birth-rate in Ontario. It developed in the discussion that not only is the birth-rate low, but the marriage-rate is far below that of any other country of equal dimensions in the world. The reason for the former was ascribed by the ministers to causes over which persons have absolute control, and they insisted that the matter involves questions of grave moral responsibility. The low marriage-rate was accounted for by the fact that so many women are employed in stores and in other occupations, and the migration of young men from the rural districts to the cities.

**A Positive Danger.**—Professor Vincenzi is said to have been making investigations regarding the purity of certain holy water used in a popular church in his native city. He found that one drop of the water taken on Saturday while people were applying it to lips or brow, when spread on a gelatin sheet, yielded within forty-eight hours about 2300 colonies of bacteria. On Sunday when the holy water was in more constant use the professor could not estimate the innumerable colonies, but his tests proved that there were diphtheria bacilli among them, and cases of diphtheria in Sassari at the time emphasized the revelation.—*Boston Medical and Surgical Journal*.

**Cornell University Medical School.**—This institution has succeeded in procuring a plot of land on the west side of First Avenue, reaching from Twenty-seventh to Twenty-eighth streets, and will commence immediately the erection of a five-story building to cover the entire plot. The building will contain not only the Medical College but also the Dispensary. The cost of the building and land is estimated at \$600,000. It is expected that the new building will be ready for occupancy in October, 1899. In the meantime the college will occupy the Loomis Laboratory and the building formerly occupied by Bellevue Hospital Medical School. The benefactor of this new institution is Colonel Oliver H. Payne who has bestowed upon it the lavish endowment of \$1,500,000.

**Diplomas Made Handy.**—The *Medical Press* has found recently in an Irish daily contemporary a congratulation to a member of the Irish Constabulary on the fact that he has become a fully qualified medical practitioner through the agency of the Queen's College, Belfast, and also, an intimation that another police constable is following his example and hopes soon to be found in "The Medical Register." Commenting upon this our contemporary says: "If the Royal Irish Constabulary permits the ab-

sence of an officer for five hours a day during four academic years, we can only express regret that we do not belong to the service. If, on the other hand, the teachers of the Queen's College and of the hospitals in Belfast are distributing sham certificates for sham attendance to enable persons who are not *bona fide* students to go in for examination, it is our duty to call the attention of the profession to the matter."

**Fifth Annual Meeting of the American Academy of Railway Surgeons.**—The preliminary program of the Fifth Annual Meeting of the American Academy of Railway Surgeons, to be held at the Auditorium, Chicago, Illinois, Wednesday, Thursday, and Friday, October 5, 6, and 7, 1898, is as follows: Wednesday, at 10 A.M.—"Anesthesia," R. H. Cowan, M.D., Radford, Virginia. "Traumatic Injuries of Peripheral Nerves," D. S. Fairchild, M.D., Clinton, Iowa. "Injuries of the Genital Organs," Milton Jay, M.D., Chicago, Illinois. Second Session at 2.30 P.M.—"The Radical Cure of Hernia," W. J. Mayo, M.D., Rochester, Minnesota. "Concealed Meningeal Hemorrhage," H. Reineking, M.D., Sheboygan, Wisconsin. "The Internment of Ericson," W. J. Galbraith, M.D., Omaha, Nebraska. Thursday, October 6th, at 9.30 A.M.—"Physical Examination for Railway Service," J. F. Pritchard, M.D., Manitowoc, Wis. "The Hygiene of Railway Injuries," G. P. Conn, M.D., Concord, New Hampshire. "Conservatism in Railway Surgery," H. Hatch, M.D., Quincy, Illinois. Fourth Session at 2.30 P.M.—President's Address, "The Higher the Order of Railway Surgery the Greater the Protection to the Employee, the Passenger, and the Company," R. Harvey Reed, M.D., Rock Springs, Wyoming. "Convenient First Dressing in Fractures, with Samples," E. H. Trickler, M.D., Cutler, Ohio. Friday, October 7th, at 9.30 A.M.—"Surgical Treatment of Some Varieties of Disease of the Prostate and Seminal Vesicles," G. E. Benninghoff, M.D. The following Fellows of the Academy have promised papers but up to date have not furnished subjects for the same: Drs. A. D. Bevan, Chicago, Illinois; Allen Staples, Dubuque, Iowa; LeRoy Dibble, Kansas City, Missouri; C. K. Cole, Helena, Montana; George W. Crile, Cleveland, Ohio. An executive session will be held daily at the opening of the meeting. D. C. Bryant, M.D., Secretary, Omaha, Nebraska.

## CORRESPONDENCE.

### THE PRIORITY IN THE "RIGHT TO LEFT AND LEFT TO RIGHT" OPERATION IN HYSTERECTOMY FOR FIBROID AND PUS CASES.

To the Editor of the MEDICAL NEWS.

DEAR SIR: December 1, 1894, I published in the MEDICAL NEWS a paper on "A New and Rapid Method of Dealing with Intraligamentous Fibromyomata." The claims I made for this operation were that by it time was saved, the uterine artery on the side of the nodule was secured beneath the nodule by a ligature applied from the vaginal side after the uterus had been cut loose from



one side and tilted over the nodule, wounding the ureter was avoided, and without splitting the capsule of the fibroid.

October 9, 1894 (before publishing this paper), I read a paper before the New York State Medical Association on "Hysterectomy in Pus Cases," in which paper I had a crude drawing showing the application of double ligatures *en masse*, placed upon one uterine artery in this manner, namely: from the vaginal side after tilting the uterus over to one side. The method necessitated removal of the cervix, the uterine artery stumps being ligated *en masse*.

At a stated meeting of the New York Obstetrical Society, December, 1894, I presented eight specimens, some pus cases, and others, pus with fibroids. The reports were not so much to illustrate the features of my operation, but rather to promote discussion regarding the propriety of the abdominal or vaginal route in operating upon these cases. Hence, these reports did not state positively that the ligatures were applied from the vaginal face in Cases II. and III.; but in Case IV. I say: "The left uterine artery was secured from the vaginal side, the fibroid nodule peeled out of the broad ligament and the uterus, together with the left adnexa, removed." I further state: "The other (feature of interest) is the removal of the specimen after the uterine arteries were secured. This latter method I have adopted now a number of times in these old inflammatory lesions where the union between bowel and pus-tube seems so intimate as would endanger the former if I attempted to remove them in face of the oozing which accompanies the separation of tissues before the vessels are secured."

So then, in October and December of 1894, I reported cases of fibroid and pyosalpinx removed by progressive ligature down one side and up the other, or "left to right and right to left." I am constrained to make claim to priority for this operation because nearly two years after the first application of this method I find in the *Bulletin of the Johns Hopkins Hospital*, February-March, 1896, a description of almost identically the same operation. The claims made by the Johns Hopkins Hospital are: (1) Saving of from sixty to eighty per cent. of the time in the enucleating stage of the operation. (2) The ease with which intraligamentary myomata and myomata beneath the pelvic peritoneum may be enucleated. (3) The ease with which inflammatory masses posterior to the broad ligament may be enucleated by attacking them from below after dividing the cervix. (4) The control of a displaced ureter on the side last opened up, keeping it out of the way of injury by the simple mechanism of the operation.

All of these claims in the *Bulletin* I had made nearly two years before. The sole points of difference are: that I do complete hysterectomy, while the Johns Hopkins Hospital staff leave the cervix; and I ligate the uterine arteries *en masse* while they ligate in continuity. The claims for the two operations are identical. As my report antedated theirs nearly two years, I claim credit for the procedure, which is known here as Kelly's operation and in France as "la méthode Américaine."

I regret very much being compelled to make this statement, and in its utterance I beg that none will think I seek to blame any one. Perhaps it is my own fault that this unpleasant situation has arisen for the *Johns Hopkins Hospital Bulletin*. I might have prevented it had I more liberally advertised and more beautifully illustrated the operation. The only agencies at my command were the New York Obstetrical Society, the *MEDICAL NEWS*, The New York State Medical Association, and my own poor pen.

Very truly yours,  
WM. R. PRYOR, M.D.

NEW YORK, September 14, 1898.

#### PRIORITY OF THE SUGGESTION OF SALINE INFUSION IN SHOCK.

To the Editor of the *MEDICAL NEWS*.

DEAR SIR: On reading my article on "Post-operative Saline Infusions," published in the *MEDICAL NEWS* of September 18th, I am struck with my omission therefrom of all reference to Dr. Dawbarn of New York, to whom the world is indebted for the first suggestion of the intravenous use of normal saline solution as a preventive of shock. Dr. Dawbarn also first demonstrated the fact that for such purpose the temperature of the solution should be high—115° to 118° F.—and thereby perfected one of our most valuable and reliable therapeutic measures in such cases.

I am so averse to doing Dr. Dawbarn injustice, even by a "sin of omission," that I ask you to publish this statement in your next issue of the "NEWS."

Very sincerely yours,  
EUGENE BOISE.

GRAND RAPIDS, MICH., September 15, 1898.

#### MONTAUK POINT AS A HOSPITAL CAMP.

To the Editor of the *MEDICAL NEWS*.

DEAR SIR: You ask my opinion as to the relation between the number of sick at Camp Wikoff and the hospital accommodations provided therefor. Premising that the opinion is formed not upon full official data but only upon such partial reports as have appeared in the newspapers and upon personal observation of a few of the regiments, I take pleasure in replying.

The camp was established for the reception of the Fifth Army Corps, then at Santiago, which numbered about 22,000 men. Several of its general officers had insisted, in a letter published in the papers, in demanding an immediate withdrawal of the entire army from Cuba because of the prevalence and rapid spread of fever among the men; they stated that 90 per cent. of the force were, or soon would be, unfitted for duty by illness. Assuming that half of the men thus affected would require hospital treatment, this estimate called for hospital care for 10,000 men. The Government began by erecting a general hospital with 500 beds and a detention hospital which ultimately had 600 beds. The general hospital was soon enlarged to the capacity of 2000 beds, and in the second week of September the four division

hospitals, each with 150 beds, were constructed; at the same time half of the general hospital and almost all of the detention hospital were taken down so that the hospital capacity of the camp was reduced at that time to about 1000 beds. Its maximum was 2000 beds, and was reached late in August. According to the scanty reports which appeared from time to time, the maximum of troops in camp at any one time was about 20,000, and was reached about September 1st. It was rapidly reduced during and after the second week of September.

The hospitals were made of floored wall tents with slight wooden frames; construction was easy and the supply could have been increased almost at will in a day or two; the supply of nurses was ample; more were offered than were accepted; that of orderlies or male nurses was apparently sufficient; that of physicians appeared to be ample. The Red Cross furnished \$14,000 worth of supplies and I believe many of the nurses.

In the first month more than 6000 patients were received in the general hospital. Almost all of them came directly to it from the transports or through the detention hospital. The average stay was less than a week probably, for the capacity was only 500 at first, and there were from 400 to 900 vacant beds after September 5th. During the rush of arrivals on transports, from 150 to 300 patients were discharged daily to make room for the new ones, and many of them were sent to hospitals in neighboring cities. Many were discharged as "well" either to their regiments or on furlough, the latter drifting in large numbers into the city hospitals a day or two after their discharge. Not a few, I have been told, were discharged against the protest of the attending physicians, and it is certain that large numbers of them still needed hospital care, and would not have been discharged when they were had it not been for the necessity of making room for others whose needs was thought to be greater. The published mortality, of about 150, is no measure of the severity of the diseases, for it does not include those who died after removal or were discharged as "well," although they and their companions are counted among the number treated. On these facts alone it is evident that the hospital accommodations were inadequate and it is probable that a number of the deaths among those who were thus discharged were due to this withdrawal of hospital care. If the condition of the troops in camp is considered this inadequacy becomes still more apparent, especially if we include among those needing hospital care those who, because of present or past illness, were unable to eat or digest the army rations, good and wholesome as they were.

That there were many really ill in the regimental camps, men with typhoid, malaria, or severe diarrhea, is beyond question, but I have no means of estimating this number. I saw some such in every regiment I visited. I have had several such in the Hudson Street Hospital (men who came to the city on furloughs and were picked up by the ambulance), and many such have been cared for in other city hospitals. Why were they not taken to the general hospital? I do not know. Probably because at first there was no room for them,

and later, because they preferred the freedom and companionship of their camp, and thought they would do as well there as in the hospital. Numerous as I believe these cases to have been, they were far outnumbered by the "ailing," those who may or may not have been on the sick-list, large as that was, who were miserable, had occasional chills, could not eat, could not work. The 8th Ohio left camp, 900 officers and men, with a sick-list of 261. The commanding officer of a regiment of regulars told me his sick list was 139 out of a total of 320. A regimental surgeon told me he had been called on to furnish 15 men for guard duty at the hospital; he picked up the best men in his regiment and before they had been two hours on duty two of them had been put to bed in the hospital. One of the papers quoted a regimental commander as saying that he could muster only 8 men fit for duty in response to a call for a detail of 24. We are all familiar with the pitiful stories told by so many eye-witnesses, of men dropping by the roadside as the troops marched from the transports to their camps; of the sick at the Montauk station, in the trains, and at Long Island City. These were the "well" men, or at least, those who did not have hospital treatment.

I would not claim that all these men, if at home in the city would receive hospital care, but they did need special food as well as medicine. Unfortunately, the Army routine makes no provision for such a condition outside of the hospital. These were men whom the War Department had collected and maintained, and drilled for months, some of them for years, in order to have an effective force of which it might at any moment have urgent need, and ordinary prudence and economy, to say nothing of humanity, should have prompted it to take fitting care of them. It was quite possible, even easy to do so. In default of special tents, those which sheltered them in the camps could have been grouped as regimental or brigade hospitals, and at the most, only extra nurses and orderlies, and the special food would have been required. Even the machinery and routine therefor could have been found in the department. The failure to do so must, I think, be deemed neglect on the part of those who were charged with the making of proper and adequate provision for the force.

LOUIS A. STIMSON, M.D.

NEW YORK, September 15, 1898.

## OUR FOREIGN LETTER.

[From Our Special Correspondent.]

RETIREMENT OF PROFESSORS ON AGE, COMPULSORY, OR OPTIONAL, AND WHEN?—ACUITY OF VISION AND ABSENCE OF BALDNESS AMONG GERMAN UNIVERSITY MEDICAL PROFESSORS—THE OPENING OF THE NEW PATHOLOGICAL MUSEUM AND THE TWENTY-FIFTH ANNIVERSARY OF THE CHARITÉ ARZTE—THE PASTEUR TREATMENT IN BERLIN—THE PADERSTEIN STIPENDIUM FOR '98 AND '99.

BERLIN, September 12, 1898.

THE recent retirement of Professor Gruber of Vienna, on the completion of his seventy-first year, recalls the question so often discussed here in Germany of making

professorial retirement obligatory in German universities at a certain age. While, thanks mainly to the efforts of the elder Rokitsansky, a law exists in Austria by which professors are retired at seventy, an additional year, the Ehren or honor year being allowed by ministerial indulgence and custom now, no such law exists in Germany. As a consequence the holder of the professor's chair to whom the blessing of years is given occupies his position on into the eighties.

While in some cases the enforced retirement at seventy-one, as is true of the distinguished Vienna otologist just now, deprives a university of some years of active professional life on the part of distinguished members of its faculty, as a rule the period of really good work is past at seventy and retirement is certainly advisable. There is no doubt that the system of non-interference in German universities has in many cases, even in great universities, left important chairs in the hands of men on whom the weight of years bore too heavily to allow of them either giving the proper encouragement to scientific advancement and initiative on the part of younger men in their department, or of suitably organizing on a modern basis the teaching that fell to their lot. I venture to say that there is not a single professorial chair in a German university occupied by a man beyond his climacteric of three score and ten, no matter how distinguished his services to science or to education have been in the past, whose retention of his position has not proved a serious hindrance to educational progress. There are cases well known where even at present special departments in great universities are woefully neglected because an old, though perhaps distinguished professor, retains a charge that should have been laid down with advancing years. So long as human nature remains our present human nature, however, the confession of senility incident to a voluntary retirement can scarcely be expected and least of all of men who have had the consciousness of their superiority to others during a long life. The necessity then for a law or custom in the matter is evident. The question is a most delicate one and the period at which retirement should be made obligatory by no means easy to agree upon. The age at which men are at the acme of their intellectual power has always been the subject of disagreement. Aristotle puts it at forty-nine and the recent graduate thinks this entirely too old, while his distinguished professor thinks it altogether too young. Not even the omniscient Aristotle ventured to suggest when men entered on the decline of their intellectual powers.

The late Professor Rokitsansky the younger, whose early death at fifty-one in his professorship at Graz deprived the scientific world of a distinguished medical man expressed some very definite opinions on the subject shortly before his death. It was mainly owing to his distinguished father's influence as I have said that Austria owes its present laws, as to the enforced retirement of university professors at seventy-one. The younger professor Rokitsansky, who had been especially interested in the question and in an excellent position to study the working of the law, urged that the age limit should be made still younger. He advocated the framing of a law requiring retirement

to the rank of emeritus professor, though with the emoluments of ordinary professor at the age of sixty-five. Only where a special faculty unanimously asked for the continuance of an individual in a professorship might the ministry or executive university authority permit service after the age of sixty-five and then for not more than two years at most.

Of course this would seem to be going to the other extreme and such a law would deprive many a German university of the services of great teachers. In Berlin, for instance, it would retire two of the great clinicians, Leyden and Gerhardt, at once, and the third, Senator within the year. These are not the only lights of German medicine who would suffer eclipse and there is scarcely a German university but would feel it. Yet the proposer of the law must have thoroughly realized this and must have been deeply imbued with the necessity for active interference in a matter of serious import to have suggested the seemingly early retirement. Meantime, however, the question remains open and an important one it would seem to be, not only of interest here in Germany, but everywhere in educational circles where the necessity has been felt of some delicate, graceful, and courteous way of shelving talent, not when its usefulness was entirely gone, but when the beginning of its decline brought with it the inevitable deficiency in initiative that makes it unsuitable for the direction of new scientific work, and incapable of imparting that enthusiasm which constitutes the best part of education.

According to a recent article in the *Deutsche Medicinische Press* of Berlin the university professors and lecturers in the medical faculties of Germany are far above the average of mortals in the matter of acuity of vision and absence of baldness. As these are two qualities not usually attributed to medical men generally and especially not to those whose devotion to science has brought them distinction in a professorial career, some of the data of the article seems of special interest.

Out of 210 professors and scientific investigators in the medical faculties of the various German and Austrian universities only 61, less than 30 per cent., wear glasses. This is, one cannot help but think, a considerably lower percentage than obtains in America; statistics in the matter would be not only interesting, but would have, I should imagine, a very practical value for ophthalmologists now that it is so usual to attribute refraction defects to over use of the eyes so much more than to a natural optic weakness. If there is any significance in a certain interrogation that is sometimes put, yet does not seem to demand an answer, who ever saw an ophthalmologist who did not wear glasses? Perhaps it would be well not to include too many of our distinguished eye specialists in the list of well-known physicians if the American statistics are to be really representative and significant. Perhaps, too, it should be the eye whose refraction required no artificial aid that should be set to the work of making out the statistics so as to be sure that lacunæ did not slip into them because of any lack of acuity of vision on the observer's part.

As to baldness the statistics are even more surprising, as



171 out of 210 professors, etc. (nearly 85 per cent.) are still in the possession of practically all their hair and in its original color. Only 21 are bald, only 18 are gray. All of them, of course, are beyond middle life, most of them being well on in years. One is tempted to wonder how many of our friends the gynecologists, whose hair troubles are proverbial in Europe as well as in America, are included in the list.

The writer in the *Medizinische Press* attributes the good eyesight of the doctors and the conservation of their hair to the fact that most of the men who distinguish themselves in medicine in Germany are men on whom great physical vitality as well as mental power has been conferred by Nature. Most of them reach an advanced old age despite years of the strictest application to science and retain their mental powers to the very end. It is getting to be more and more generally admitted in all lines of intellectual work that genius, or at least talent, is practically always associated with a natural substratum of intensest vitality. *Mens sana in corpore sano* is not only a maxim for practical guidance in the matter of physical exercise in after life but is an original condition on which serious scientific work is dependent for its success primarily. Hence, the normal retention of even unimportant natural advantages despite a life of serious application.

Already the first inklings of a noteworthy medical celebration to be held in Berlin at the beginning of next year are coming to light. The *Charité* Ärzte, the association of consultants and residents at the *Charité* Hospital, Berlin, will celebrate their twenty-fifth anniversary early next year. The new Pathological Institute, at least the Pathological Museum Department, is to be finished by that time and is to be formally opened then. As many of the prominent medical men in most of the German universities have at some time or other in their career been in service at the *Charité* the assemblage of members will form a distinguished reunion. The celebration will practically take on the character of an inauguration of the New *Charité*, of which this will be the first department opened. The occasion is the twenty-fifth anniversary of a most important medical society and the initiation of that new era which, in a completely remodeled *Charité*, is to assure Berlin the position she holds, of European center for medical teaching, and is expected to inspire a good deal of enthusiasm for the celebration and the event will be one that will attract more than passing notice in the medical world.

The special feature of therapeutics in Berlin continues for the moment to be the treatment of rabies by the Pasteur method. Within six weeks after the opening of the new department some dozen of cases had been under treatment. They are practically all cases from the German provinces near the Russian border. They are not, however, the result of any sudden irruption of the disease into German territory. When such cases occurred before this they were sent either to St. Petersburg or Vienna. The expressions of those in charge of the department continue to be those of eminent satisfaction. The dangers of the treatment once so much insisted on in Germany are now

considered to be minimal or non-existent. Already movements are on foot for the establishment of similar departments in other university cities.

The medical faculty of the University of Berlin announced at the end of the summer semester that the Paderstein stipendium for the next year was conferred on Dr. Max Koch, a graduate of Berlin. His graduation thesis, of which the stipendium is practically a mark of recognition, was reviewed in these columns just after its publication. Its subject, "The Connection between Atrophy of the Glands of the Gastro-intestinal Mucous Membrane and Pernicious Anemia," is of living interest just now and the review of the immense literature to date, with the actual observations of the writer himself make it of great practical value though his conclusions are but those that have already been gradually gaining acceptance that the changes noted and for a time thought so important are only adventitious, the result not the cause of the anemia.

## SELECTION.

### THE USE AND ABUSE OF MIDWIFERY FORCEPS.<sup>1</sup>

I.—BY R. MILNE MURRAY, M.A., M.B., F.R.C.P. (Edin.), Lecturer on Midwifery and Diseases of Women, Edinburgh School of Medicine; Assistant Physician, Royal Maternity Hospital.

*Surgical Midwifery.*—I think I am right in stating that the selection of this subject for discussion arose out of the able and trenchant address delivered by the former President of this Section at the Montreal meeting last year. In that address Dr. Sinclair drew attention to what he considered the close relation between many gynecological troubles and the over-frequent and injudicious use of the forceps.

The address contained much to give the reader pause, but its general tenor may be gathered from a brief quotation from it which I venture to make: "Midwifery has during the same period," says Dr. Sinclair, "become also largely surgical—too surgical, and a thesis which I shall endeavor to maintain to-day is, that gynecology has become so largely surgical as the direct result of surgical interference in midwifery practice; the accoucheurs are the providers of material for the gynecologists. . . . The term surgical is employed here with almost exclusive reference to the midwifery forceps."

I am not using too strong an expression when I say that this amounts to an indictment of the present practice of midwifery all over the world; and coming as it did from the Presidential Chair of this Section, it must have caused much searching of heart on the part of accoucheurs on both sides of the Atlantic.

But it was not only on the general practitioner that the

<sup>1</sup> The discussion held by the Section on Obstetrics and Diseases of Woman of the British Medical Association at its recent meeting in Edinburgh, slightly abridged from the report as it appeared in the *British Medical Journal*. A condensed abstract made by our special reporter was published in the *MEDICAL NEWS*, August 13, 1898. The subject is so important and the opinions of so many eminent men are given that the discussion is deemed worthy of fuller presentation.—Ed.

strictures of the address reflected. If the practice of midwifery were aberrant, no small part of the blame must lie at the door of the teachers of obstetrics. If the practice were loose, the teaching must be greatly at fault.

It could not be supposed that such a statement could be made by one in the position Dr. Sinclair occupies without due deliberation, because it directly implied that one section of those whom he was then addressing was engaged in providing employment for the others, either by reason of gross and culpable error of judgment or by direct malpractice.

*The Value of the Forceps as a Surgical Instrument.*—I shall best fulfil my duty to-day by raising points for discussion, or suggesting the lines which the discussion should take, rather than by attempting any necessarily inadequate deliverance on the whole subject.

I begin by asking, "What is the special value of the forceps as a surgical instrument?" To Baudelocque is due the statement that the midwifery forceps is the most valuable surgical instrument that has ever been invented, and "with this statement," says Dr. Sinclair, "we are all more or less in agreement." I think there are few here who will cavil at my reply to the question I have just put, if I say that the special value of the forceps is that it enables us to shorten the suffering and diminish the risk of childbirth. If it is to justify the claim of Baudelocque it must accomplish these results in a sensible proportion of cases, because if it is only to be employed at rare intervals to diminish the suffering or even save the life of an occasional mother, while we should cherish it as a valuable component of our armamentarium, we would scarcely place it in the position claimed for it by Baudelocque. Apart, however, from the question of the frequency of its proper use, I may take it for granted that its value consists in the power it gives us on certain occasions of shortening the duration of labor, of diminishing the dangers of the mother, and greatly increasing the safety of the child.

But when we turn to the scope of the instrument and ask what are the indications for its use, we reach highly debatable ground. I daresay if the question were put to twenty individual members of this meeting there would be given twenty distinct answers, all more or less divergent, and some diametrically opposed. Perhaps the one side would best be represented by the answer Dr. Sinclair would give, while that of his anonymous friend, who startled him with the statement that his forceps cases amounted to seventy per cent. of his whole midwifery practice, would adequately represent the opposite; and these two, along with the other eighteen, would give one more instance of the many-sided character of the human mind!

*When Forceps Should Be Used.*—I shall make no attempt to analyse or classify those hypothetical answers. It will be more useful if I try to lay down a general principle which ought to guide us in seeking the indications for the use of this instrument. I think I can be sure of taking you all with me when I say that *a direct indication for the use of the forceps arises whenever—and only whenever—we are assured that the danger of interference has become less than that of leaving the patient alone.*

So simple is this statement, that to some it may seem a mere truism, and to be scarcely worthy of deliberate enunciation. Nevertheless, I believed it is, in the case of some practitioners, just one of those pious opinions which are assented to the moment they are stated, and the moment after practically ignored.

I venture to think it is more than a mere truism, and that it ought to be kept more constantly before us and more strictly acted on than it sometimes is. For let me point out that it implies that the use of the forceps, in practically every case, is a matter of individual judgment. There is no accepted set of rules which can be applied to each emergency, and which will decide for us in each case whether we are to act or to hold our hand. It is for this reason that the question of the proper use of the forceps is often so difficult. Much more than in most surgical operations have we to consider in each case, not one, but many circumstances. If we discover an abscess, or a cancer, we deal with them at once by opening or excising them according to fixed principles, and by methods which have been determined by experiment and settled by experience. But the operation of the forceps comes under a different category. A fetal head and an ovarian tumor cannot be dealt with on the same lines. It is sound practice to remove the latter as soon as it is detected, but the mere presence of a fetal head in the pelvic cavity is not an adequate reason for its summary removal. There are many things to be considered, many things to be taken into account—the state of the passages, the efficiency of the pains, the condition of the child—ere we can justify any effort to supplement or supersede the processes of Nature. Each case must be decided on its merits, and in every case when the question of forceps arises, we must ask, "Is it safer to act or to wait?" We have no justification for interference until we have decided that the danger is in waiting, nor, let me add, in failing to act when we have decided that the danger is in delaying. It is this necessity for individual judgment in every case which renders the employment of the instrument a matter of grave responsibility.

*Premature Use of Forceps.*—From what I have said, it will follow that a certain number of the instances of improper use or abuse of the forceps are due to errors of judgment—the instrument is employed when it should not be used; or else it is not used in cases when it is indicated. It was to the frequency with which the first error is committed that Dr. Sinclair specially devoted his address last year, and no doubt a very substantial proportion of the instances of the abuse of the forceps comes under this category. But this does not by any means represent all the cases of misuse of the forceps; for while injury is inflicted in many cases by the premature use of the instrument, and in a smaller number by undue delay in its employment, a very large part of the injury to mother and child—and thus a large part of what may be properly termed the abuse of the forceps—is due to faulty technic.

I may perhaps best put it thus: (a) The forceps are often used at the wrong times. (b) They are sometimes not used at the right times. (c) They are often badly

used at both times. The danger of the forceps operation, apart from the risk of septic infection (which, of course, is common to most operations), may be summarized thus:

1. The mother's parts may be bruised, lacerated, or otherwise injured by mechanical violence.
2. The too sudden emptying of the uterus may be followed by imperfect retraction, and, consequently, dangerous hemorrhage.
3. The fetal head may be unduly compressed, lacerated, or otherwise damaged.

Now, in spite of all that has been done and is being done to diminish the risk and suffering of disease by anesthesia, antiseptics, and other means, there seems a lingering doubt in the minds of some as to whether the abolition of the pain and suffering of labor would be an unmixed blessing. This seems to me to be a survival of the same barbarous instinct which prompted the so-called religious objection to the use of chloroform in labor, and is as unworthy of respect. For myself I have no such scruple, and were I able to do so with safety I should gladly abolish every pain after the first, and reduce the duration of labor to a bare minimum. But unfortunately this is not practicable. Physiological labor consists of a series of phenomena which require time for their development; and a grave abuse of the forceps consists in their employment in such a way as to interfere with the proper sequence of the normal physiological events.

*Forceps in Undilated Os.*—This leads me to consider a group of cases in which we have the most striking illustration of the use of the forceps at the wrong time, namely, their application through an undilated os. There is, no doubt, a small minority of cases in which such an application is, for special reasons, justified and even demanded; but these are few and far between, and the disastrous results following their use in many of these cases are only too familiar to obstetricians. The statistics of this practice, obtained from the results of the Rotunda Hospital under Dr. Johnston's mastership, are in themselves a commentary on the dangers both to mother and child.

*Spasmodic Rigidity.*—I would draw particular attention to the dangers involved, because I feel sure there is a group of cases falling under the term "undilated os" which is not so generally recognized as it should be, and the non-recognition of which seems to me to account for more damage to the mother's parts than we may at first admit to be likely. Every one is familiar with, and no one can well overlook, the ordinary form of undilated os, in which, either from early rupture of membranes, narrow pelvis, malpresentation, or other cause, the os remains for hours in a half dilated state. If, in spite of such a condition, the use of forceps is for some reason, good or bad, decided on, the operator recognizes the condition, calculates the risk, and, it is to be hoped, does what he can to minimize or obviate that risk to the utmost of his power by careful management. But there is another form of undilated os quite as unsuitable for the forceps operation as this, and which

is the more dangerous because it is not always recognized. I am convinced it accounts for the occurrence of by far the larger number of cases of lacerated cervix. For while the delivery of the head through the variety of undilated cervix referred to is always a difficult and often a dangerous operation, nevertheless, it can be done, and is done in many cases, given a proper instrument, the requisite skill, and abundant patience. It is not so with the variety I now wish to consider. Here is the sort of case I refer to. A woman has been in labor for some hours; probably the membranes have ruptured early, and dilatation is slow. Opium possibly has been given, or chloroform has been used, and after a time an examination, made in the absence of a pain, shows the os soft, flabby, and apparently nearly the size of the child's head. The head is probably well down in the cavity, and lying fairly well for the application. Here, then, seems a suitable case—a long labor, a tired woman, perhaps, if I may whisper it, a wearied accoucheur, and most important of all, an os, if not dilated, at least dilatable. A little more chloroform is given, and then without any difficulty the forceps are slipped over the head and, of course, inside the os. By this time a uterine pain, excited by the manipulation, begins, and if the os is now examined it will be found spasmodically contracted round the head and forceps, and not more than half its former apparent size. Unfortunately, it sometimes happens that an examination is not made, and traction is commenced with results which do not require description here. The case is one of partially dilated, spasmodically rigid os, and may be described in physiological language as an example of uterine incoordination, or, popularly, as a uterine stammer. The os during an interval is soft, flabby, and easily stretched; during a pain it becomes tense, hard, and greatly narrowed.

I have several times been asked to see patients in consultation, where the attendant thinking the os was fully dilated, had ruptured the membranes, and found after uterine pains had begun again, that the os was less than two-thirds dilated. The same mistake had arisen—the os was examined in the intervals, and its spasmodic rigidity overlooked. To use the forceps here and to treat the case as if the os were dilated or dilatable is one of the gravest, and, I fear, only too common examples of their abuse. On account of the tension and thinness of its edge, very slight efforts of traction will tear the cervix into the vaginal roof, inflicting frightful injury.

*Secondary Inertia.*—I need not detain you by dwelling on the dangers of the application of the forceps to a woman in a condition of secondary inertia, and to the almost inevitable results in the shape of deficient retraction and hemorrhage in the third stage and *post-partum*. And I do not doubt that other instances of the use of the instrument at the wrong time will be sufficiently emphasized by one or other of those who follow me in this discussion.

*Failure to Use Forceps When Necessary.*—Let me then say a word about the second point I raised; namely, that the forceps are sometimes not used at the right time.



Whatever material damage or injury may be inflicted by the injudicious use of the forceps, there is one injury—once common enough, and now rarely to be seen—namely, vesicovaginal fistula, whose disappearance is in great measure a result of the earlier, perhaps often premature, application of the forceps. No more striking instance of the beneficent use of the forceps can be adduced than that which we may point to in the almost total disappearance of this frightful injury. Most men nowadays take care that the long, tedious, grinding pressure of an ill-placed head, on a contracted or otherwise modified pelvis, is never allowed to occur, and the consequent necrosis and sloughing are thus largely avoided.

But I cannot help thinking that the reasonable reaction which has shown itself in many schools against the too frequent use of the forceps may in some measure diminish our vigilance in this matter, and that from an unwillingness to interfere we may unwittingly render possible an undue amount of maternal bruising and other damage. I could not find a better illustration of what I refer to than I do in the "Rotunda Report" for 1896-97. Here there is a record of 56 forceps cases, and in the summary of the indications of their use I find that 45 were treated by forceps on account of "delay in the second stage of over four hours." In the absence of further statistical detail I cannot express an opinion as to whether the forceps in these 45 cases were rightly or wrongly employed, whether it might not have been better to have interfered sooner than the fourth hour in some cases, or to have waited even longer still in others before artificially finishing the labor. The report simply states that "forceps were applied in cases of delay of four hours in the second stage with material benefit to the mother and greater safety to the child, and we consider the results obtained, more particularly as regards the life of the child, amply justify us in not delaying longer" (p. 10).

The objection I would make to a statement such as this in an authoritative report is, that it gives us an impression that the element of time is the primary determining factor. I am quite at a loss to understand why, for example, we should wait in every case for four hours after the os is dilated and when the pains are not advancing the head. To my mind the latter point is the *crux*. If I could assure myself that after dilatation of the os the advance of the head was blocked—either by the size of the head, the resistance of the canal, or the feebleness of the pains—I should not wait a moment before using the forceps. I could not assure myself that this was the case without waiting a certain time, but I am perfectly certain that in many cases I should reach absolute assurance long before four hours had elapsed. If I am certain of this in one hour, why should I wait three more? What do I gain, and what does the patient gain, by my watching her enduring two or three hours of further misery and suffering? If the head is in the brim and shows signs of descending, I shall wait till it descends, because the operation of forceps is easier in proportion to its descent. If the head is in the oblique diameter and shows signs of rotating, I shall certainly wait till the rotation is complete, because the application of forceps is simplest and the de-

livery safest when the head is in the anteroposterior. But if neither of these things occurs from fault or failure of any factor, I see nothing to be gained, but much to be lost in waiting after I am certain that progress is at an end. The danger of interference is then less than the danger of waiting.

There are many wise obstetric aphorisms, but none wiser than this: "Wait till you see what Nature can effect—not what she can endure." No one need imagine that this can be read as an apology of meddlesome midwifery. As long as Nature is preparing or opening the passages, and molding or advancing the passenger, she is doing what she can do better than any of us, but she sometimes fails, and it is because she fails that Baudelocque's commendation of the forceps is "one with which we are all more or less in agreement," because by its means we have saved women hundreds and thousands of weary hours of useless suffering and preserved countless children alive. Let it not be supposed that I am traversing the practice of the Rotunda Hospital. The report at my disposal gives me no facts on which I could base my criticism; what I suggest is that the mode of stating the grounds of interference is essentially misleading, and I wish to enter my protest against the idea which it conveys that the time element alone is a proper basis for such interference.

If I am asked when, under such conditions, I should apply forceps, I should make a reply in words which I first heard from the lips of my respected teacher and your distinguished President, and which I have never forgotten: "When the passages are in a fit state and Nature fails to advance the head, then I should apply the forceps."

*Forceps in Occipitoposterior Positions.*—I must leave to others the indication of other cases in which the forceps might be used with advantage when their use is avoided or delayed. But I should like to direct attention to one group of cases in which the forceps might be employed with greater discrimination than is, I think, sometimes used. I refer to occipitoposterior positions of the vertex. We all know that in such cases, when flexion is well marked and when the pains are efficient, descent and forward rotation is the most usual ending, while in those cases in which flexion is deficient, descent is difficult and backward rotation the rule. My own view and teaching in the matter is this—that when flexion is deficient, as shown by the ease with which the bregma can be reached, the sooner forceps are applied after the os is dilated the better. In view of the practice generally recommended in regard to these cases this may seem heroic if not rash. But I confess I have little faith in the attempt at manual rotation; my experience of it in these cases is that it is futile, and on theoretical grounds I have never understood how it can be anything else. When it succeeds it seems to me almost certain that rotation would have occurred without it. For if natural rotation is the result of the action of the pelvic floor on the advancing head, I cannot understand how twisting the head in the canal can develop it when it is defective. If in rotating the head we brought it to bear more firmly on the floor, I could understand that a sound principle was involved,

but we can scarcely claim that we do this. Everything, however, depends on the forceps employed. With the axis-traction forceps, more especially if slightly modified, we can develop flexion and secure rotation in nine out of ten cases, and in the tenth would probably have done so too had we used them soon enough.

*Faulty Technic.*—Finally, the forceps are abused by being badly used; not only when they should not be used, but also in cases in which their use is indicated. Now I am convinced that to these faults of technic, as I may call them, are due not only the greater number of minor errors in the use of the forceps, but likewise some of the gravest examples of maternal and fetal injury. I venture to refer to a few of them—some of them I regard as of most importance.

The first fault of technic to which I ask attention is one which is still too common. It consists in not employing what is indubitably the most efficient instrument. If a workman chooses for his purpose a tool which is less efficient than some other which is available, he may execute the work, but he does so either less perfectly than he might, or does so with an unnecessary expenditure of time and energy. So if the problem to be solved in the forceps operation is to extract the child with the expenditure of as little force as possible, we fail in that problem if we do not use the instrument which enables us to economise our force to the greatest extent. That this instrument is one constructed on the principles first laid down by the late Professor Tarnier has been long taught by every member of the school to which I belong. Professor Simpson has illustrated the value of the axis-traction forceps by saying that a man who has once used them would as soon think of reverting to the old forceps as a man who could keep a carriage would think of conducting his practice on foot.

When I was a boy I was more or less familiar experimentally and otherwise with a piece of mechanism, the prototype of the modern bicycle appropriately and familiarly known as a "boneshaker." It possessed wheels, a backbone, a saddle, and a steering-bar, just like its descendant, and among other things it had a tire which, if it had no other advantages, was at least durable and puncture-proof. It certainly was a very different affair from the machine on which we see young and old, rich and poor, gliding along noiselessly and almost without effort at five to fifteen miles an hour. Now I should be inclined to say that a man is just as likely to revert to the old instrument after having used the axis-traction forceps as he would be to give up a good bicycle of 1898 for a boneshaker of the early Seventies'. The main argument advanced in favor of the older forceps is its simplicity, and this argument is quite as much in favor of the boneshaker, for its construction was complicated neither by ball-bearings, spring saddles, nor pneumatic tires. But I have heard a practitioner declare that he could do as much with the old forceps as another could with the axis-traction forceps. I would as soon believe that the average man on the primitive bicycle I have referred to could ride as far or as fast, and with as little fatigue, as one on a modern machine.

*The Axis-traction Forceps.*—To object to the instrument that it is complicated is an argument with which, I confess, I have little patience. That a couple of rods and a handle-bar should present a serious difficulty is an argument scarcely worthy of a grown man.

I may take it that practically it is universally admitted that axis-traction is of great advantage at the brim, and with regard to its efficiency there I need not detain you. The pleasure I have of knowing that many of those I address constantly employ and strongly advocate these instruments is saddened by the knowledge that there are some who listen to me who are not yet convinced of their efficiency, save at the brim. But I am certain of this, that those who have systematically used the instrument in all their forceps cases will bear me out that for once they have proved their efficiency at the brim, they have done so ten times in the cavity and twenty times at the outlet. They will speak of the security of the grasp, the absence of dangerous compression, the ease of extraction, the delicacy with which the proper axis can be ascertained and followed, and the safety to the soft parts which it ensures. They will perhaps, most of all, commend the instrument on account of the facility with which it produces or permits of the production of rotation.

Compare this with the deliverance on the axis-traction forceps I find in one of the recent text-books on midwifery. "The use of the axis-traction forceps is practically confined," says the author, "to pulling the head through a contracted brim; when the head has passed into the cavity it is no better than the ordinary kind. It is only of use, therefore, to pull the head through a somewhat smaller brim than can be passed by the latter. In cases where the difficulty is so great as this, it is in all probability better to turn." This reduces Tarnier's work to a vanishing quantity. And again, in another part of the same book, I find, "they all (that is, axis-traction forceps), however, have the same disadvantage of compressing the head during their application."

I confess it is difficult for one who has used these forceps for nearly twenty years to treat such comments seriously. As to the question of compression, I do not believe that the compression of the head by the axis-traction forceps is as much as ten per cent. of that inflicted by the ordinary forceps. Any one can prove to himself that after the blades are in position and in contact with the head, one-half turn of the screw is all that is necessary to keep them in position. Not infrequently the screw slips out of its catch during traction, and I have delivered many cases in which the screw was off all the time the forceps were in the cavity. When the head is in the cavity, the pressure of the walls, the inward pull of the rods, and the cephalic curve of the blades keep the latter in perfect security on the head. With the ordinary forceps, it is obvious that the harder you pull the more you grip the handles—not in order to hold the head, but to prevent the handles from slipping from your grasp; nevertheless, the head bears the brunt of the pressure.

As to the efficiency of the instrument in the cavity, I have said all that I need say, and as to removing the forceps when the head has reached the perineum, for myself

I should as soon think of leaving the patient in the third stage of labor as of removing the forceps before complete delivery of the head. I am prepared to maintain that in axis-traction forceps, properly constructed and properly used, we have not only a means of accomplishing delivery in cases of delay with safety and success, but by their means we are able to avoid laceration of the pelvic floor in cases in which this would unavoidably occur if left to Nature. For I have long held that deep anesthesia, combined with the skilful use of the axis-traction forceps, is the best treatment for rigid perineum.

*The Guiding Principle in Applying the Blades.*—The second fault of technic to which I shall direct attention is in connection with the application of the blades. Of course when the head is at the outlet and fully rotated, the application can only be faulty from gross carelessness; but when the head is in the cavity and still in the oblique diameter, a very faulty application is often made. It is still largely the teaching and the practice of the British schools to apply the forceps as far as possible in relation to the pelvic transverse without reference to the position of the head. Now when the head is high in the cavity, or even low in the cavity and not rotated, I am convinced that this is wrong. The result of this practice is that we obtain a more or less oblique grasp of the head, and it follows that, since the blades tend to rest on somewhat sharply curved portions of the head, one or the other inclines to slip off during application, and the difficulty of locking is notorious. Equally notorious is it that the vicious practice of trying to rectify this by twisting the blades into position is a source of danger to mother and child. But apart from this, after locking is effected and traction begun, one of two things follows:

1. The head as it descends rotates, and in doing so throws the forceps into the opposite oblique. This usually happens with axis-traction forceps when rotation is favored. If delivery is effected without removing and readjusting the forceps, extensive damage to the outlet will follow.
2. The head descends without rotating, and engages the outlet in the oblique. This often happens with the older forceps, and is even more disastrous than the former.

I venture to think that the practice of the French school is much sounder. The forceps should be applied to the biparietal diameter of the child's head wherever situated, and then, as the head descends, rotation will bring the blades into the transverse diameter just as the head enters the anteroposterior, and the head, forceps, and pelvis will present proper relations at the outlet. I feel sure that extensive damage to the lower part of the vaginal canal—especially internal laceration of the perineum—is often caused by oblique position of the head or of the forceps at the outlet. The application of the forceps to the side of the head may require more care than the other methods, but we are there to take what care is necessary for the benefit of the patient.

I need not dwell on the objectionable results of jerky irregular traction. A uterine pain begins almost imperceptibly and increases in intensity by fine gradations. So

should an effort of traction. Only by this can we bring the head and canal into proper relation, and only by this can we develop rotation and minimize the danger of pressure.

*The Mode of Traction.*—Every year I become more and more convinced of the advantages the axis-traction forceps give us in protecting the perineum from injury caused by undue delay, or laceration caused by faulty mechanism. The procedure I adopt is as follows: As soon as the vault touches the floor the forceps are held between the finger and thumb of the right hand, while the left is placed over the fundus. I may say at once that I never in any circumstances "support the perineum." The left hand enables me to control the retraction, to detect the beginning of a pain, and often to aid the action of the forceps by efficient pressure. I press the chloroform as far as will keep the patient quite still. Extraction is effected by gentle pressure on the traction-bar, the rods and shanks being kept just touching. With certain exceptions I act as far as possible only with a pain taking care always that traction is effected in such a way as to maintain the advance of the suboccipito-bregmatic diameter. In this lies the secret of success, for by means of these forceps we can restore or maintain the relation of this diameter to the outlet—a relation which is often disturbed by peculiarities of the pelvis, and more often by alteration in the shape of the head, caused by molding or swelling of the scalp, associated with long and difficult labor.

There is one group of cases, however, which form a marked exception to the rule of extracting with a pain. Not infrequently we find in elderly primiparæ that every uterine pain, when the head is low, is accompanied by a spasmodic action of the muscles of the floor, which for the time narrows the vulvar orifice and throws the perineum into a state of intense rigidity. It is in cases of this type that we find instances of perineal laceration. In such a case I deepen the anesthesia and carefully avoid traction, save in the intervals of pains. Delivery is simple and easy thus, for the orifice is often wide enough when not spasmodically contracted. This is another instance of incoordination, and must be treated in this special way. Without forceps such cases will continue in labor for hours, and as the spasm only passes off when the pains are checked by chloroform, this alone is not enough.

*Conclusion.*—Whether the forceps are to be an engine of disease and death, or a means of protection alike to mother and child, will depend upon whether they are used with a seeing eye and an understanding heart—upon whether the obstetrician is prepared to put aside all adventitious considerations, "the uneasiness of the gossips"—even the demands of the patient, most of all his own convenience, and interfere when, and only when, he has deliberately assured himself that the risk of interference is less than the danger of waiting. If this principle is unswervingly followed and carried out with reasonable skill, Baudelocque's commendation will still hold good, and the forceps will maintain their high place among the products of the ingenuity of man.



II.—W. S. PLAYFAIR, M.D., LL.D., Consulting Obstetric Physician, King's College Hospital, and Emeritus Professor of Obstetrics in King's College.

*Over-action and Reaction.*—Dr. Playfair said that he had always believed that the surgical development of gynecology was due to such things as ovariectomy, removal of the appendages, hysterectomy, and the like, which no one could say were produced by the accoucheurs; but granting that it was only a harmless exaggeration, it was none the least necessary to reply to it. It must at once be admitted that Professor Sinclair's address would do a great deal of good. It would warn against excess, it would teach medical men to be more careful than many had been. But did that touch the principle? There was no improvement in medicine which was not open to the accusation of having been abused. Take trachelorrhaphy, which Professor Sinclair himself cited as an example of a great improvement in gynecology. Surely the use in England of that occasionally valuable operation had been greatly hampered by the ridiculous way in which it had been overdone in a sister country. Again, with the very useful "rest cure," had not the abuse of massage made that very useful remedy stink in the nostrils of right-minded people? So with the most recent fashionable remedy—the "Schott cure"—and many others which could be named. All this should warn against abuse, but it should not be used as an argument against proper use. Most of those present were probably entirely unaware of the frightful evils which followed the old practice. His old friend and fellow student, Professor Simpson, would remember how common in their student days was that awful accident "vesicovaginal fistula," now hardly ever seen, its abolition being the direct result of modern practice. Again, Dr. Sinclair had not even alluded to one of the great glories of modern midwifery—the almost total abolition of craniotomy, formerly so common as to be an every-day procedure. Surely this was an enormous gain. Who could say how many lacerated cervixes and torn perineae should be taken to be equivalent to even one human life saved from the deadly perforation? At any rate he, Dr. Playfair, must insist that his book on midwifery should not be blamed for any such over-frequent use of the instrument, since he had most carefully safeguarded his teaching on this point.

III.—W. J. SMYLY, M.D., Late Master of the Rotunda Hospital, Dublin.

*The Time to Use Forceps.*—Dr. Smyly said that safety in the use of the forceps lay in adhering as closely as possible to their employment under favorable conditions, that is, when the head presented and had passed the brim by its greatest transverse diameter, when the os was fully dilated or dilatable and the membranes ruptured. Sometimes it was necessary to employ the instrument under less favorable conditions, but then the indications must be more definite than when, as under the conditions indicated, they were employed merely to save suffering. Under unfavorable circumstances they should only be employed in case of danger to the mother or child. The high forceps was, therefore, an operation not to be taken lightly in hand, and the importance of time in molding the

head to the brim should not be forgotten; a head might come through with molding which could not have been brought through with forceps at an earlier stage. Time was of no account in midwifery nor any indication for the use of forceps.

IV.—PROFESSOR FEHLING of Halle.

*Frequency of Forceps Operations.*—Professor Fehling, speaking as a teacher, said that the chief mistake of students and practitioners was to employ forceps in the absence of strict indications, hence the bad lacerations seen were due to the increasing frequency of forceps operations; while, as Hegar and Cullingworth had shown, the mortality in childbed had not been diminished. In ordinary cases three points must be insisted on: the head must be under the brim, well rotated, or dilated; if so, then the forceps might be employed when there were indications in the condition of mother or child. If there were any danger for one or another, it was justifiable to operate in the absence of these three conditions. He wished to emphasize the advice that students should be taught not only on the phantom, but in the labor-ward. At Halle, during the last four years, the forceps had been used in 290 out of 3000 cases, but only 55 of these were in the clinic (3.5 per cent.); the greatest number occurred in the polyclinic. Two mothers died from eclampsia, two from rupture, which had occurred before the forceps were applied; of the children, 6.2 per cent. were still-born. Most of the operations were done by the students, the others by the assistants. Professor Fehling felt sure that to have done one operation under the supervision of the master prevented the young physician from making mistakes later on in practice.

V.—SIR WILLIAM PRIESTLY, M.D., M.P., Consulting Obstetric Physician, King's College Hospital.

*Meddlesome Midwifery.*—There had been of late years such improvements in the construction of forceps and in the recognition of the suitable conditions for their use that obstetricians might well feel more confidence in their value, and there could be no doubt that a great saving of life and of injury had been effected both to mothers and children.

The danger perhaps now was lest the use of forceps should be pushed too far, and it was well that conservative influence should be exercised to restrict them within proper bounds. He was glad to recognize that the reader of the paper gave a timely warning in this respect. It was still true that "meddlesome midwifery" might be bad practice, but this did not necessarily mean that forceps must not be used more frequently than formerly, always supposing that the best forms of instruments were selected, and that the conditions at the time of their application were such that there was the smallest possible risk of injury to both mother and child.

To apply forceps before the passages were sufficiently dilated, and especially when the os uteri was imperfectly dilated or rigid, was to incur great risk of laceration, and there was then so much more force to overcome that the attempt might well fail. It was quite true, as had just been stated, that the natural forces would often mold the fetal head to the maternal passages in such way as

to facilitate delivery when forceps applied above the brim had failed to effect delivery.

VI.—WM. STEPHENSON, M.D., Professor of Midwifery in the University of Aberdeen.

*Preventive Midwifery.*—Use and abuse must be taken together, and the second term made to limit that intended to be included in the first. Frequent use and abuse was the idea. Frequency in the use was the great advance in midwifery during the latter half of this century. It was well to remember that the movement originated in the ranks of the profession, and not from the teachers. He would prefer that the general practitioner would be heard to-day, and not the teacher. He could not accept the rule adopted by some leading teachers—that instrumental aid was to be given only when there were symptoms of danger to mother or to child, or to both. It was the old cry: Wait till Nature begins to fail. The true principle was: Aid whenever you can without deranging the mechanism, so as to avert symptoms of danger. But Dr. Murray had fully vindicated the present frequent use against the fallacies and strictures of those who would have men return to the custom of a hundred years ago. Dr. Stephenson further discussed the point that with the axis-traction forceps there was a delicacy of manipulation, which a skilled man, while retaining the use of the ordinary handles, could exercise more especially in aiding rotation in O.-P. positions. He found it, however, impossible to discuss that question in full on that occasion.

VII.—ARTHUR V. MACAN, M.B., F.R.C.P., King's Professor of Midwifery, Trinity College, Dublin.

*The Principle of Administration.*—I would first wish to explain Dr. Playfair's observations about Dr. Labath's statistics of his cases of craniotomy during his mastership of the Rotunda Hospital, which he said were so numerous that it was not considered worth while to keep any statistics of them. This is not so, but each master takes the records of his mastership away with him, and unless he has published them no one else can supply them.

Next as to the dictum that Dr. Milne Murray has given us, namely, that the forceps should be applied when the danger of delay is greater than the danger of delivery by the forceps. This seems quite simple, and at once gains our assent. But when we try to translate it into practice we find that it requires great skill and experience to know when the danger of the operation is less than the danger of delay. It is, therefore, my practice to impress on my students the fact that the chief danger of delay lies in want of antiseptics, and that if they were careful of the latter the want of the former is less important.

Personally I consider that the greatest triumphs of the forceps are the cases that require the least traction. If the force of resistance is the least greater than the driving force there can be no progress. Here the slightest traction with the forceps will effect delivery, and this seems to me the greatest triumph.

Before going on to the axis-traction forceps I would like to point out that the ordinary double-curved forceps can be used—indeed, ought always to be used—as an axis-

traction instrument. Traction in the direction of the handles of the double-curved forceps is always wrong, and traction by both hands, one at each end of the handle, drawing the latter in a series of parallel lines toward the chest of the operator, is essential to making the instrument an axis-traction instrument. If the axis-traction use of ordinary double-curved forceps were properly taught it would be quite unnecessary for the instrument-maker to stamp on one of the blades of the forceps, "Left, lower, first." This seems to me to presuppose a very low standard of intelligence and knowledge of the instrument in the man that is about to apply them.

Nor can I agree with the view that the forceps should not be applied to the head until the largest diameter of the head is past the brim. For in such cases, if the head is already fixed to the brim, and the indications for delivery are urgent, the only alternative is craniotomy. Indeed, in cases of flattened pelvis it is not infrequent to feel the head being flattened inward and springing outward, according as the traction is applied or relaxed, and on examining the head afterward we find a spoon-shaped depression on the child's head, caused by the pressure of the sacrovertebral angle against the head. The axis-traction forceps which we use in Dublin is that introduced by my friend Dr. Neville, which, as far as I can see, is quite as effective, and much more simple and easy of application, than any other axis-traction forceps with which I am acquainted.

VIII.—J. M. MUNRO KERR, M.B., F.F.P.S.G., Assistant Professor of Midwifery, Glasgow University; Assistant Physician, Glasgow Maternity Hospital.

*Mode of Grasping the Head.*—I desire, owing to the enormous number of questions involved in such a discussion as the present, to confine myself entirely to one point and it is to an abuse, or rather misuse, that I wish to refer. It is the general teaching, and it will be found in the text-books of Galabin and Playfair, that forceps should be applied without reference to the diameter of the child's head. Now, I consider this bad teaching, because there is a lack of exactness in it. The forceps should be applied with reference to the diameter of the child's head. If the head is low down in the pelvis, it is always possible to grasp the head transversely. If the head is high up it is more difficult; with care, however, even in these cases the head can be grasped nearly transversely, and the more nearly transversely it is grasped the better. If it be admitted—and I think it will be—that the more nearly transversely the head is grasped the better, then the general teaching I have referred to is defective, because it will be a chance whether or not the head is grasped in the best manner possible. Further, if cases of flat pelvis be considered, it is of the very greatest importance to have regard to the child's head and grasp it exactly anteroposteriorly. A slight departure from such a grasp will very readily result in injury to the child and injury to the mother from the forceps slipping.

IX.—J. W. BYERS, M.D., Professor of Midwifery and Diseases of Women and Children, Queen's College, Belfast.

*Cases Requiring Forceps.*—Professor Byers thought that all would be agreed that the indications for the use of the forceps had been well put by Dr. Milne Murray: "Wait until you can see what Nature can effect; don't interfere until she fails." When danger arose either to mother or child, it was desirable to act at once; but there were other cases in which there was no doubt that, even in the absence of distinct evidence of danger, all other conditions being favorable, the use of the forceps was justifiable. He was entirely in favor of the use of axis-traction forceps, and it was only fair to Dr. Milne Murray to say that it was to this type of instrument he alone referred in the discussion. He was with those who said that full dilatation of the os was necessary before using the forceps, but in certain exceptional cases (as in eclampsia or sometimes in aged primiparæ) they might be called for earlier. As to using forceps at the brim, he preferred to apply them rather than to turn in a flat pelvis. As to the use of the forceps where the head had come to the perineum, he preferred not to take them off, but to continue their use until the head was fully delivered, but the point was to let the head come out not, as the majority of books taught, by extension, but in a gliding manner.

X.—W. JAPP SINCLAIR, M.D., F.R.C.P., Professor of Obstetrics and Gynecology, Victoria University, Manchester.

*Unskilful Use of Forceps.*—Dr. Sinclair expressed modified approval of the axis-traction forceps, but could not imagine a time when the ordinary long forceps would be discarded in favor of the axis-traction instrument. His address in Montreal had been misunderstood to some extent, and he proceeded to speak of Dr. Playfair's criticisms. He reiterated a statement of his position in regard to the use of forceps and the teaching of midwifery. There was at least sufficient ground for reconsidering current practice, and the forceps must be used unskilfully as long as they were used by men who had not received exact teaching so as to diagnose the position of the head before the instrument was used.

XI.—ROBERT JARDINE, M.D., Physician, Glasgow Maternity Hospital.

*The Axis-traction Forceps in Contracted Pelves.*—Dr. Jardine said that in Glasgow they had to deal with a very large number of contracted pelves. Last year they had ten per cent. of these cases requiring major operations. Like Professor Byers, he believed axis-traction forceps gave a better chance to both the child and mother. He had delivered children weighing  $8\frac{1}{2}$  lbs. alive through a true conjugate of three inches, and he always used the forceps in preference to turning. There was one precept to which he attached importance—never to turn after failure with axis-traction forceps. He had never seen a child saved in this way, but he had had cases of rupture of the uterus brought into the hospital which had been caused by this. Craniotomy was not an obsolete operation as some of the former speakers had intimated. In cases of this kind it should be done. In regard to putting on the instruments before the os was fully dilated, he said there were a few cases in which it was necessary, but such cases were very few. If the patient were under chloro-

form, it was, as a rule, possible to dilate manually. Theoretically the transverse grasp of the child's head was the right one, but practically it was often impossible, and he had seen over and over again that when the child was delivered the grasp was oblique. With a good instrument like Dr. Milne Murray's, it mattered not what the grasp was, as there would be no damage done to the head.

## REVIEWS.

A SYSTEM OF MEDICINE BY MANY WRITERS. Edited by THOMAS CLIFFORD ALLBUTT, M.A., M.D., Regius Professor of Physics in the University of Cambridge. Volume VI. New York: The MacMillan Co., 1898.

THIS volume, the last but one in this superb system, is devoted to the respiratory organs, the pleura, and the circulatory system. The diseases of the arterial system are omitted because of a delay on the part of Dr. Welch of Baltimore on "Thrombosis and Embolism." The editor, in a rather humorous way, apologizes for this author's delinquency, which is attributed to his work, never to be forgotten, against the antivivisection bill now pending in Congress.

If anything this volume is even better than its predecessors. A detailed review is impossible in these columns, but a few of the leading articles will be mentioned. Dr. Pye-Smith writes in a scholarly way on pneumonia, giving the minutiae of its bacteriology in the clearest possible manner. The veteran Dr. Samuel West has an article on intrapleural tension full of clinical value. Professor Foster has written a lucid article on the general macroscopic and microscopic features of the blood, and the editor contributes monographs on chlorosis, functional diseases of the heart, mechanical strain of the heart, and aortic diseases. An interesting contribution is that of Dr. Thomas Oliver on the effects of electric currents of high pressure, the author attributing deaths by electricity to sudden cessation of the heart's beat. Dr. Robert Muir's paper on leucocythemia is admirable and is beautifully illustrated.

The volume is marked by literary excellence as well as scientific accuracy and well bears out what we have previously emphasized—that Allbutt's system is a classic of the highest grade.

THE JOHNS HOPKINS HOSPITAL REPORTS. Report in Gynecology. Baltimore: The Johns Hopkins Press, 1898.

THIS volume contains but two articles. One is an exhaustive analytic study of seventeen hundred cases of abdominal section from the standpoint of intraperitoneal drainage, by Dr. J. G. Clark. This work is already familiar to readers of gynecologic literature, and it is necessary only to mention the author's conclusions, based on wide reading, abundant clinical experience, animal experimentation, and autopsies. Mechanical drainage he condemns and offers in its place so-called postural drainage obtained by elevating the foot of the patient's bed, salt solution of a normal strength having been introduced



into the abdominal cavity before this has been closed. The saline fluid, gravitating toward the diaphragm where it is absorbed, drains the abdomen of fluids which are the result of operative interference or of disease which it is impossible to remove by mere mechanical means. For the details of the technic the reader should refer to the original article which is well worth the careful study of every abdominal surgeon.

The second paper is by Dr. James E. Stokes, on "The Etiology and Structure of True Vaginal Cysts." The author points out the difference in structure and descent of cysts found in the posterior wall, the lateral walls, the vault of the vagina, and shows that such cyst may arise from inclusion, from embryonic peculiarities, or from the glandular structure of the vagina. The paper is well illustrated by histologic drawings.

**THE OFFICE TREATMENT OF HEMORRHOIDS, FISTULA, ETC., WITHOUT OPERATION.** By CHARLES B. KELSEY, A.M., M.D., Late Professor of Surgery at the New York Post-graduate Medical School and Hospital. New York: E. R. Pelton, 1898.

AFTER reading this little volume, one wonders why these short clinical lectures were ever put into book form. The first of them tells what most surgeons know, that the packing, and sometimes the curetting of fistulæ, will lead to a cure. The second emphasizes the fact that the rectal specialist should be an abdominal surgeon and capable of performing plastic operations on the female, which is laudable. The third and last speaks of the abuses of colostomy and urges that such operations shall always be done after due deliberation, which is not remarkable. The author takes occasion as well, to refer to his own low mortality after this operation, which is commendable.

**DISEASES OF THE STOMACH.** A text-book for practitioners and students. By MAX EINHORN, M.D., Instructor in Clinical Medicine in the New York Post-graduate Medical School and Hospital; Visiting Physician to the German Dispensary. Second revised edition. New York: William Wood & Company, 1898.

THE first edition of Einhorn's "Diseases of the Stomach," published less than two years ago, was received with such favor that it at once occupied the niche among authoritative text-books on this side of the Atlantic that Ewald's treatise on the same subject holds in Germany. This second edition differs but little from its predecessor, although a few slight additions and alterations have been made.

The book is divided into fourteen chapters, the subjects being treated in a thoroughly scientific manner and sufficiently in detail to allow of the student obtaining an exact working knowledge of each. The first chapter deals with the anatomy and physiology of the stomach, and then follow in turn others on methods of examination, diet, local treatment, organic diseases with constant lesions; functional diseases with variable lesions; abnormal conditions with reference to size, shape and position; nervous affections, and the condition of the stomach in diseases of other organs.

Einhorn's work in connection with direct electrization of the stomach is so well known that the chapter on this subject will be read with particular interest. His very large experience with the method, as perfected by himself, and the almost uniformly good results obtained, have placed the procedure in the list of those upon which the general practitioner as well as the specialist can rely with complete confidence in those cases in which it is indicated.

It is unnecessary to refer more in detail to Dr. Einhorn's book. This second edition will but enhance its popularity, and it is safe to assume that the edition will be exhausted within a period as short as was the first.

## THERAPEUTIC HINTS.

### *For Fissures of the Nipple.*—

The following method of treatment is recommended as effecting a cure within three or four days:

Cleanse the nipple with a one-per-cent. bichlorid solution and dry with sterilized cotton. Cover fissures with thiol-collodion (ten per cent.), avoiding the openings of the milk-ducts. Order the use of a nipple-shield until healing is completed.

*For Tropical Dysentery.*—According to observations made on the island of Ceylon, ATTYGALLE testifies to the satisfactory results obtained by the administration of chlorid of ammonium every four hours, with the addition, if indicated, of opium and cannabis indica. The patients were given a diet of milk and arrow root. In the majority of cases the stools ceased to be bloody, and the colicky pains disappeared after three or four days of this treatment.

*Tonic Pills in Locomotor Ataxia.*—For the tonic treatment prescribed after a course of mercurials the following formula is used by ERB:

R	Ferri lactatis . . . .	gr. xl-gr. lxxx
	Ext. cinchonæ . . . .	3 i-gr. lxxx
	Ext. nucis vomicæ . . .	gr. v-gr. xv
	Ext. gentianæ, . . . .	q. s.

M. Ft. pil. No. XL. Sig. One or two pills three times a day after meals.

*Salicylic-Acid Ointment in Acute Rheumatism.*—Since a small amount of salicylic acid is absorbed through the skin, its external application is recommended as a valuable adjunct to internal medication, and as a substitute for the latter when improvement begins. Its advantages lie in the relief afforded the stomach and in the suggestive action of the local treatment. The turpentine should be omitted from the following formula as soon as the superficial epidermis is destroyed:

R	Ac. salicylici	} aa . . . . . 3 iiii
	Ol. terebinthinæ . . .	
	Lanolini	
	Adipis . . . . .	q. s. ad. 3 iii.

M. Ft. ungt. Sig. External use. Cover with a dry bandage.—*Sternberg-Lods.*